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THE QUARTERLY REVIEW of BIOLOGY

SOME PRINCIPLES IN THE THERMAL REQUIREMENTS OF FISHES

By J. R. BRETT

Pacific Biological Station, Nanaimo

INTRODUCTION

THE tissues of a fish, permeated by the network of the circulatory system, are constantly being brought into equilibrium with the external temperature at the gill surface. The high specific heat of water, which efficiently buffers the aquatic climate from the extremes of aerial conditions, has chained the gill-respiring vertebrate to a cold-blooded existence. To achieve regulation of body temperature requires control of the temperature at the respiratory surface or of the circulatory fluid, as well as of the body tissues. Insulation of the gills by means of fat deposition would defeat their primary purpose for oxygen uptake. Alternatively, the energy exchange required to bring the volume of water necessary for adequate respiration up, or down, to a satisfactory body temperature would frequently be exhausting. Evaporation, the chief physical aid for heat loss in an air-breathing vertebrate, is denied the gill-respiring vertebrate; adaptations to an aquatic life leave it entirely subject to the variations of environmental temperature. Measurements of the body temperature of fish and other aquatic poikilotherms confirm this relation, the equilibrium state being reached more rapidly among small than large forms (S. Simpson, 1908; Pearse and Hall, 1928; Nielson, 1938; Gunn, 1942; Davis, 1955).

Minor exceptions may arise through heat production from excessive activity, particularly for a fish caught by some means which prevents normal respiration, or through the rapid excursion from one level of thermal stratification to another (Morrow and Mauro, 1950). At near-lethal, low temperatures the effect of greatly reduced respiration may permit body temperatures to be temporarily maintained above that of the environment. Measurements of the stomach temperature of brown bullheads (*Ameiurus nebulosus*) when taken from 20°C. and placed in ice-water showed an internal temperature of 0.3° to 0.5°C. above the external temperature after two hours of immersion. (Previously unreported observations made at the Ont. Fish. Res. Lab., Algonquin Park, Ont.)

Lacking a means of maintaining an independent body temperature, the aquatic poikilotherm may be a victim of rapid internal temperature change caused by sudden changes in environmental temperature. The response of many fish when plunged into relatively cold water is one of violent bursts of activity followed by benumbed inactivity. Such a temperature change, although acting as a strong stimulus, simultaneously depresses the animal into temporary dormancy. The conflict of combined stimulation and depression is quite striking. Because of the all-pervading nature of environmental temperature, the fundamental thermal requirement of fishes is an *external* environmental temperature most suitable to their

internal tissues. That this temperature may not be limited to a matter of a few degrees attests the extent to which the poikilotherm has been able to evolve bodily functions which can maintain adequate roles *despite* variation in body temperature (cf. Bullock, 1955).

Temperature sets lethal limits to life; it conditions the animal through acclimation to meet levels of temperature that would otherwise be intolerable; it governs the rate of development; it sets the limits of metabolic rate within which the animal is free to perform; and it acts as a directive factor resulting in the congregation of fish within given thermal ranges, or movements to new environmental conditions. Some of the principles in this multiple role of temperature will be considered.

LETHAL LIMITS

Upper and lower lethal temperatures

The upper and lower limits of temperature which fish can withstand define the extremes of tolerable thermal environment. While a variety of experimental procedures have been used to determine the lethal limits (Loeb and Wasteneys, 1912; Hathaway, 1927; Fry, Brett, and Clawson, 1942; Doudoroff, 1942), in each case an effort has been made to provide a simple index, usually expressed as the mean or median temperature tolerance for an unselected sample of fish when exposed for some predetermined interval of time. A number of distinct characteristics of the lethal temperature have been repeatedly demonstrated. Under carefully controlled conditions the upper lethal temperature is remarkably precise. It can be determined consistently in many species to within ±0.2°C. This sensitivity has permitted a comparison of species tolerance, with significant differences obtained between closely related species (Brett, 1952) and some subspecies (Hart, 1952). Indeed, Gibson (1953) has obtained data which suggest that temperature resistance in the guppy (*Lebistes reticulatus*) can be affected by selective breeding. In addition, species which show similar upper lethal limits may be characterized by quite different lower levels of tolerance. The order of temperature resistance (heat-tolerance) among eight species of freshwater fish from Algonquin Park lakes, Ontario, was reversed for half these species when they were tested for cold-tolerance (Brett, 1944).

The lethal limits are only consistent within a species when repeated for fish from similar acclimation temperatures and cultured under comparable conditions (cf. Hoar and Cottle, 1952a; Gibson, 1953). As the temperature of acclimation increases, so the lethal temperatures progressively increase. For the goldfish (*Carassius auratus*) Fry et al. (1942) report that for every 3°C. rise in acclimation temperature the rate of change of the upper lethal temperature was about 1°C., and that for the lower lethal temperature nearly 2°C. A considerably lesser rate of change was obtained for yearling speckled trout (*Salvelinus fontinalis*), which required a 7°C. change in acclimation to produce a 1°C. change in the upper lethal temperature (Fry, Hart, and Walker, 1946). The rate of increase is not constant throughout the full range of acclimation, but usually "levels off" to zero in the region of relatively high acclimation for each species. The fact that the lethal temperatures are subject to change through acclimation has led to the calculation of the area enclosed within upper and lower lethal ranges, for temperatures of acclimation from 0°C. to the maximum for any given species (Fig. 1; Table 1). This defines the *zone of tolerance*, with the unit of tolerance equal to the area bounded by 1°C. (expressed as "degrees Centigrade squared"). The extent of eurythermicity of the organism

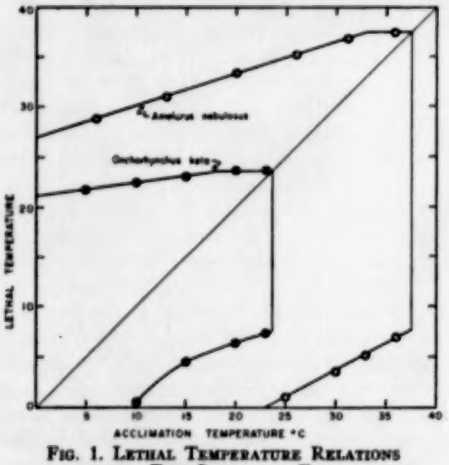

FIG. 1. LETHAL TEMPERATURE RELATIONS FOR TWO SPECIES OF FISH

The bullhead, *Ameiurus nebulosus*, is a highly tolerant species in contrast to the chum salmon, *Oncorhynchus keta*, which is one of the least resistant studied to date. The area enclosed by each trapezium is the *zone of tolerance*. (Data from Brett, 1944 and 1952).

TABLE 1

Lethal temperatures and thermal tolerances of various species of fish arranged in order of thermal tolerance

Species	Family		Upper (U.) and Lower (L.) lethal temperatures for acclimation temperatures of:						Thermal tolerance	Habitat	Reference
			5°C.	10°C	15°C	20°C	25°C.	30°C			
*Oncorhynchus gorbuscha**	Salmonidae	U.	21.3	22.5	23.1	23.9			450‡	Cool streams	7
		L.									
*Oncorhynchus keta**	Salminidae	U.	21.8	22.6	23.1	23.7			468	Cool streams	7
		L.		0.5	4.7	6.5					
*Oncorhynchus nerka**	Salmonidae	U.	22.2	23.4	24.4	24.8			505	Cool streams, deep lakes	7
		L.	0.0	3.1	4.1	4.7					
*Oncorhynchus kisutch**	Salmonidae	U.	22.9	23.7	24.3	25.0			528	Cool to moderately warm streams and lakes	7
		L.	0.2	1.7	3.5	4.5					
*Oncorhynchus tshawytscha**	Salmonidae	U.	21.5	24.3	25.0	25.1			529	Cool streams	7
		L.		0.8	2.5	4.5					
Salvelinus fontinalis	Salmonidae	U.	23.7†	24.4†	25.0	25.3	25.3		625	Cool streams, deep lakes	4
		L.					0.5				
Perca flavescens	Percidae	U.	21.3	25.0	27.7		29.7		742	Deep and shallow lakes	5
		L.		1.1			3.7				
Notropis atherinoides	Cyprinidae	U.	23.2	26.7	28.9	30.7	30.7		747	Shallow lakes, cool streams	5
		L.			1.6	5.2	8.0				
Catostomus commersonni	Catostomidae	U.	26.3	27.7	29.3	29.3	29.3		770	Shallow lakes, warm and cool streams	5
		L.				2.5	6.0				
Rhinichthys atratulus	Cyprinidae	U.	26.5	28.8	29.6	29.3			790	Warm and cool streams	6
		L.				2.2					
Girella nigricans	Stromateidae	U.		28.7		31.4		31.4	800	Shallow marine bays and pools	2
		L.		5.0		8.0		13.0			
Notropis cornutus	Cyprinidae	U.	26.7	28.6	30.3		31.0	31.0	803	Shallow northern lakes. Cool and warm streams	5
		L.			0.0	3.7	7.8				
Semotilus atromaculatus	Cyprinidae	U.	24.7	27.3	29.3	30.3	30.3		808	Lake shallows, warm and cool streams	6
		L.				0.7	4.5				
Dorosoma cepedianum	Clupeidae	U.					34.3	35.9	880‡	Shallow, open water	6
		L.					10.8	14.5			
Hyborhynchus notatus	Cyprinidae	U.	26.0	28.3	30.6	31.7	33.3		884	Shallow lakes, slow streams	5
		L.			1.0	4.2	7.5				
Pimephales promelas	Cyprinidae	U.		28.2		31.7		33.2	903	Shallow lakes	5
		L.				1.5		10.5			
Lepomis machrochirus	Centrarchidae	U.			30.7	31.5		33.8?	910‡	Shallow lakes	6
		L.			2.5	5.0	7.5	11.1?			
Notemigonus crysoleucas	Cyprinidae	U.		29.3	30.5	31.8	33.2	34.7	940	Warm and slow, cool streams	6
		L.			1.5	4.0	7.0	11.2			
Micropterus salmoides	Centrarchidae	U.				32.5	34.5	36.4	965‡	Marsh and shallow reed beds	6
		L.				5.5		11.8			
Ictalurus lacustris	Ameiuridae	U.			30.3	32.8	33.5		970‡	Large rivers and warm lakes	6
		L.			0.0	2.5	6.0				
Gambusia affinis	Poeciliidae	U.			35.4	37.3	37.3	37.3	1110‡	Marsh, very shallow reed beds	6
		L.			1.5	5.5					
Ameiurus nebulosus	Ameiuridae	U.	28.6	30.2	31.8	33.4	35.0	36.5	1162	Marsh; shallow zone of lakes	3
		L.				−1.0	1.3	3.7			
Carassius auratus	Cyprinidae	U.	29.0†	30.8	32.8†	34.8†	36.6†	38.6†	1220	Very shallow water	1
		L.				2.0	6.0	9.0			

* Freshwater fry stage only.

† Estimate from graph by interpolation.

‡ Provisionary, pending more data.

References: (1) Fry, Brett, and Clawson, 1942; (2) Doudoroff, 1942; (3) Brett, 1944; (4) Fry, Hart, and Walker, 1946; (5) Hart, 1947; (6) Hart, 1952; (7) Brett, 1952.

can thus be readily determined. Hart (1947) ascertained the lethal effects of temperature for eight species of fish from the Toronto region and made comparisons with results reported elsewhere in the literature. Since then Hart (1952) has published further studies on lethal temperatures; and results on temperature tolerance in young Pacific salmon have been reported by Brett (1952). These data are summarized in Table 1. Some generalities can be derived.

The Salmonidae have the lowest thermal tolerance found to date, with maximum upper lethal temperatures barely exceeding 25°C. The sea-migratory forms (*Oncorhynchus*) have considerably less resistance to low temperatures than is indicated for the lake and stream-dwelling char (*Salvelinus fontinalis*); indeed the latter has a greater cold-tolerance than any species yet reported.

With the exception of the goldfish (*Carassius*), the Cyprinidae occupy a somewhat intermediate position, with thermal tolerances ranging from levels approaching but distinctly above the Salmonidae, the difference resulting more from a resistance to high than to low temperatures. Through acclimation, an upper lethal temperature exceeding 30°C. is achieved by most members of this family.

The thermal tolerances of the single members studied from the families Percidae, Stromateidae, and Catostomidae occur at the low end of the range for the cyprinids, whereas the two centrarchids are towards the upper end.

The Ameiuridae appear to occupy the top position among the most eurythermal, along with the goldfish. The common bullhead (*Ameiurus nebulosus*) is also particularly resistant to low temperatures.

Black (1953) has made a comparison of upper lethal temperatures for similar and closely related species studied in British Columbia with those from Ontario (Brett, 1944; Fry et al., 1946; Hart, 1947) and some parts of the United States (Hart, 1952). Although variations can be seen to exist, these cannot be entirely ascribed to different geographic locations, owing to slight differences in experimental technique and the question of exact acclimation temperatures. However, the general conclusions are essentially the same as established elsewhere. One recurring exception appears to exist in the case of the largemouth bass (*Micropterus salmoides*), which Hart (1952) also reported to show significant geographic differences. It would be of interest to culture eggs taken from these separate populations under identical conditions and to establish unequivocally the degree of inherent physiological difference.

Resistance times

The lethal temperature is theoretically conceived as that temperature which fifty per cent of a population could withstand for infinite time. Experimentally it is obtained by subjecting samples of fish to a series of temperatures, higher levels of which result in complete mortality of the sample. The period of tolerance prior to death is known as *the resistance time* (Fry, 1947a). By plotting the logarithm of the median resistance time (i.e., the resistance of the most representative fish) for each of a number of test temperatures, the points are seen to form a linear series, the slope of which is relatively consistent for most species of fish, despite very different levels of resistance (Fig. 2). In each case an abrupt change in slope occurs, marking the point of transition from *resistance* to *tolerance* and signifying that death from temperature as a primary cause has ceased. This temperature is the *incipient lethal temperature*.

By virtue of this resistance it is possible for fish to make excursions for limited times into temperature zones which would eventually be lethal.

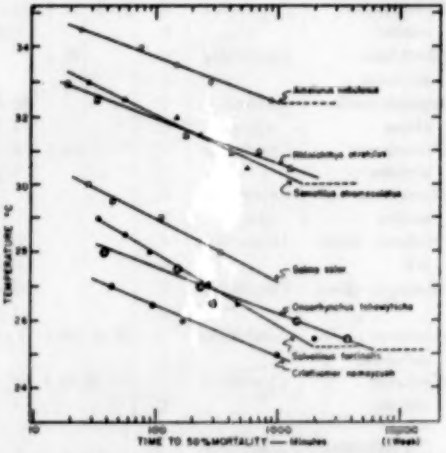

FIG. 2. MEDIAN RESISTANCE TIMES FOR SEVEN SPECIES OF FISH ACCLIMATED TO 20°C.
Broken lines indicate incipient lethal level. (Data from Fry, 1947b; Hart, 1952; Brett, 1952).

It also permits them to tolerate some diurnal fluctuations in which the peaks of environmental temperature actually exceed the lethal temperature (Fry et al., 1946).

Possible cause of death

The life of an animal can be maintained only in conjunction with the continuous functioning and integration of the majority of its tissues. The thermal lethal level must be set by the resistance of the most sensitive essential tissue. This tissue, for upper lethal temperatures, appears to be that of the nervous system. Freeman (1950) discovered that the metabolic activity of goldfish brain tissue approached zero at a temperature approximating the ultimate upper lethal temperature reported for this species by Fry et al. (1942). A pronounced depression, though not a cessation, in the rate of oxygen uptake of excised brain tissue from the largemouth black bass is reported to occur at 35°C. (Fuhrman, Hollinger, Crimson, Field, and Weymouth, 1944). The bass had been collected in a reservoir with surface temperatures ranging to 24°C. It is of interest that Hart (1952) obtained an upper lethal temperature of 34.5°C. for this species when acclimated to 25°C. In some fish the cause of death from high temperatures may possibly result from the inactivation of the respiratory center, followed by death from oxygen lack. The respiratory movements of the yellow perch (*Perca flavescens*) cease entirely with the approach of heat-death (Brett, 1944). Battle (1926) subjected muscle and nerve tissue of the skate (*Raja radiata* and *R. erinacea*) to gradual heating. Inactivation of the nerve tissues preceded that of the somatic and smooth muscles.

Death from intolerably low temperatures has also been attributed in some instances to failure of the nervous system. However, it is now known that thermal death may not always arise from the same cause. Doudoroff (1942) distinguished stages of "primary" and "secondary chill-coma" among samples of *Girella nigricans* when subjected to low temperatures. "Primary chill-coma" was not manifest until some seconds after immersion and apparently did not stimulate the cutaneous sense organs but penetrated to the central nervous system. A similar phenomenon was observed among young Pacific salmon (Brett, 1952). Experiments performed in saline solutions, slightly hypertonic to blood concentrations, indicated that three causes of death from low temperature were involved: one, a very rapid agent, usually effective within 60 minutes of exposure and probably a disturbance of the central nervous system; a second, which was related to osmotic balance; and a third, delayed in time of onset, and of unknown origin.

Peiss and Field (1950) studied the oxygen consumption of excised brain and liver tissues from the cold-adapted polar cod (*Boreogadus saida*) and compared these with similar studies on the warm-adapted golden orfe (*Idus melanotus*). The polar cod, obtained from waters ranging from −1.5°C. to +2.0°C., showed an ability to maintain a consistent rate of oxygen uptake of its tissues at all temperatures from 0°C. to 25°C. That of the orfe, although fairly similar for higher temperatures, was considerably reduced at levels below 10°C. In contrast, the period of sustained tissue respiration was progressively curtailed above 15°C. in the cod, whereas the orfe was capable of maintaining its uptake at all temperatures studied. In both cases the oxygen demand of brain tissue was greater than that of liver tissue. The authors concluded that the tissues of the polar cod were advantageously adapted to cold. Adjustment in the metabolic demand of the total organism to the appropriate climatic conditions has been studied by Scholander, Flagg, Walters, and Irving (1953). A comparison between some arctic and tropical fishes indicated that the cold-adapted forms were likewise capable of a higher rate of oxygen uptake at comparable low temperatures. When actively swimming, the arctic fishes expended a greater proportion of their available energy per unit time than did the tropical forms. That is, the routine act of locomotion is maintained much nearer the maximum level possible among arctic than tropical fishes at their respective normal environmental temperatures.

Hoar, in conjunction with Dorchester (1949) and later Cottle (1952a, b), has examined the effect of dietary fats of different melting points on the temperature tolerance of gold-fish, and made an effort to ascertain the nature of the thermal death of tissues. Although significant changes in resistance to high and to low temperatures were demonstrated, no direct relation between tolerance and the melting point of ingested fats was obtained. These authors believe that the integrity of the plasma membrane and its permeability is associated with the presence of unsaturated lipids in the structure of the membrane.

If the limiting tissue responsible for death from extremes of temperature is that of the nervous system, a critical examination of changes in this tissue separately is warranted.

Rate of acclimation

The varying ability among fishes to extend their temperature tolerance through acclimation has been illustrated. While the principle of acclimation, or more generally acclimatization (used synonymously with acclimation, cf. Bullock, 1955), has been recognized since the last century (Davenport and Castle, 1895), surprisingly little study has been devoted to this phenomenon. It would appear that the rate of increase in ability to tolerate higher temperatures among fish is relatively rapid, requiring less than 24 hours at temperatures above 20°C. (Doudoroff, 1942; Brett, 1944). Conversely, the loss in this increased tolerance, and the gain in resistance to low temperatures, are inherently slower processes, requiring up to 20 days in some species to approach completion. These rates appear to be governed by the rate of metabolism, which, if depressed by a low environmental temperature, automatically reduces the rate of acclimation. Whatever the process, Brett (1944) discovered that acclimation to a higher temperature in the bullhead was inhibited under conditions of low oxygen concentration.

It must therefore be a characteristic of at least the most sensitive tissue, or the integrating system (e.g., myoneural junction, nerve synapse), to be capable of rapid and reversible changes in temperature tolerance. Such a fish as the bullhead can increase its heat-tolerance by 10°C., whereas the speckled trout is limited to less than 5°C. Although fish lack a thermal control mechanism, their capability of inhabiting a wide range of thermal environments has been achieved by the development of remarkably tolerant tissues, and has been extended through acclimation. It is possible to culture goldfish over a range of 40°C. The homoiotherm has sacrificed this tissue tolerance, supplanted as it is by the considerably increased limits of tolerance to extremes of environmental temperature which thermal control has provided for the intact organism.

Ecological significance

There can be no doubt that the lethal temperature exerts limiting effects on the geographic distribution and freedom for successful existence in the confines of lake and stream, or the strata of the ocean. Fry (1951) has assembled much of the existing evidence for the environmental relations of temperature in the speckled trout (cf. Creaser, 1930; Ricker, 1934). The agreement between field and laboratory findings is convincing. Crises, when temperatures reach 24°C. to 27°C., can be survived only for short periods (Huntsman, 1946). However, the ability of fish to survive near-freezing temperatures is annually demonstrated in ice-covered lakes.

Somewhat in contrast to these are the temperature relations for six species of freshwater fish from lakes in Algonquin Park, Ontario (Brett, 1944). Seasonal changes in the upper lethal temperatures were determined for samples of fish from locations in which recording thermometers had been placed. In every instance the lethal temperatures rose rapidly with ascending lake temperatures, exceeding the daily lake maxima by 6° to 8°C. The highest seasonal lethal temperatures recorded for the six species ranged from 32°C. to 35°C., yet the full scope for temperature tolerance had not been tapped, as laboratory experiments revealed. The ability to exceed considerably the highest environmental temperatures to be expected in the common range of the species signifies that lethal relations must play a relatively unimportant role in the limiting of many hardy species, at least in the post-embryonic stage. Yet, in their general order of increasing heat-tolerance, they show a correlation with an ecological succession from cold streams and deep lakes to warmer streams and littoral zones, and finally to marsh conditions (Hart, 1952; Table 1, this paper). Some other property or attribute of the organism, roughly reflected by the upper lethal temperature, must be operating as the restricting factor to distribution.

Few cases of heat-death among fish are recorded in the literature (Bailey, 1955). Cold-death, however, has been reported more frequently (Verril, 1901; Storey, 1937; Gunther, 1941; Galloway, 1941). Although the threat to existence appears to come more from conditions of low temperature than of high temperature, the correlation between lower lethal temperatures and habitat is even less apparent than for upper lethal temperatures. This may be attributed in part to a characteristically slow rate of gain of cold-tolerance among fishes. It is the *rapidity* of the onset of low temperatures

that probably causes death, outstripping the ability of the fish to acclimate to a lower level, despite being within its biokinetic range.

Severe cold in the North Sea, as reported by A. C. Simpson (1953), appears to have exceeded the lower lethal temperatures for plaice, cod, whiting, and dab despite the probability that time did permit maximum acclimation. Nevertheless the author relates that "continuous cold easterly gales caused rapid cooling of the surface water." The lowest surface temperature recorded was −1.8°C. and a probable minimum bottom temperature of 0°C. (Vaux, 1953). The freezing point of the body fluids of teleosts lies mainly within a range of −0.5°C. to −0.9°C. (Dakin, 1912; Krogh, 1939; Kubo, 1953).

The state of thermal acclimation of the bullhead was traced by Brett (1944; 1946) from May to September in Lake Opeongo. The ability of this species to achieve a high level of acclimation as the environmental temperature rose rapidly in spring provided adequate safeguard for the high temperatures in the shallow reed beds of its habitat. As a result of the rapid acclimation to high temperatures, coupled with a slow loss of this recently acquired tolerance, acclimation kept pace with the daily lake water maxima.

The inability among young salmon to cope with osmotic stress at low temperatures would suggest that the estuarian conditions of moderate salinity would provide some sanctuary from cold-death.

DEVELOPMENTAL STAGES

As has been suggested, extremes of temperature can be withstood by many fishes to a degree which considerably exceeds the normal peaks of their environmental temperature; and habitat correlations with lethal temperatures are but grossly apparent. This applies only to the free-swimming, fully-formed fish. Precise knowledge of thermal tolerance and, more generally, the thermal requirements for the egg, developing embryo, and larval stages is scant (cf. Bonnet, 1939). There are inherent difficulties in determining the effects of temperature on an organism during a period of such rapid cellular change, particularly in the light of the importance of acclimation.

For animals in general, Baldwin (1948) comments that "at certain periods, notably at the time of gastrulation, the developing embryo is particularly susceptible to thermal and other changes." Battle (1929) discovered that the early blastodermic stages of the rockling (*Enchelyopus cimbrius*) were most susceptible to heat death. Abnormalities were produced by lowering the temperature of the ova to 5°C. Embody (1934) and Hayes (1949) have conducted research and reviewed much of the available information concerning the effects of temperature on salmonoid eggs. Interest has been centered on the incubation period and growth relations within a viable range of temperatures, embracing extremes of 1.5°C. (speckled trout) and 17.6°C. (rainbow trout). There is nothing to indicate that normal development is not possible throughout this span. The growth relation, however, shows some inconsistencies at the extremes. The theoretical relation of "*temperature* (°C.) × *time* = *constant*" applies quite well from 4 to 14°C. Deviation at lower temperatures would suggest that hatching is possible at 0°C. Embody, referring to the work of Dannevig (1894), notes that eggs of a marine fish, the cod, "were partly or wholly developed in sea water over a temperature range from −1 to +14°C."

Salmon and trout hatcheries are usually located near a natural source of cold, clear, running water. The necessity for low temperatures in artificial propagation, however, arises as much from avoiding disease under the unnaturally crowded conditions, as from obtaining a suitable temperature for development and hatching. Undoubtedly the migrations to cool, freshwater streams are necessary for the survival of the salmon species, from a thermal as well as an osmotic requirement. The embryonic stage of many coldwater fishes is passed during the winter and early spring, the fry emerging as the temperatures begin to rise. Relatively uniform low temperatures prevail. Pacific salmon (*Oncorhynchus*) eggs have been found to be viable under thin sheets of ice, locking the surface gravel like cement. When death occurs it has frequently been attributed to desiccation, oxygen lack, or disease rather than low temperature, although such a diagnosis is usually speculative.

It can be stated at least that the thermal requirements in the very early stages are more exacting than in the adult.

Ketchen (1953), while studying factors affecting the survival of the lemon sole (*Parophrys vetulus*) in Hecate Strait, B.C., discovered an inverse correlation between year-class strength and water temperature at the time of pelagic life. By relating

drift currents with temperature conditions in the Strait, the possibility of high temperatures so accelerating development that the young fish metamorphosed prior to reaching appropriate rearing grounds was demonstrated (cf. C. J. Fish, 1927; Walford, 1938). This interesting speculation exemplifies a more subtle limiting effect of temperature in combination with a second environmental factor.

Profitable comparison can be made with another cold-blooded vertebrate, the frog, from studies by Moore (1949) on some of the physical factors affecting distribution and survival in the genus *Rana*. In correlating adaptive differences with environmental temperatures, some of the main divergence among the species occurred in embryonic temperature tolerance and rate of development. The latter was greater at lower temperatures for species with a more northern distribution. This highly significant work on *Rana* points up the lack of such knowledge among other poikilotherms. Among amphibia in general McFarland (1955) has commented that "the effect of temperature acclimation on amphibians has been studied but little; in fact in the entire class little is known about lethal temperature limits."

A study of the stages in thermal requirements during development in fishes is sorely needed.

TEMPERATURE AND ACTIVITY

Death from temperature intolerance marks the ultimate breakdown in the organization of the individual. However, it is well recognized that feeding, resistance to disease, successful reproduction, and sufficient activity to permit existence in the face of competition and predation are all necessary for the continued maintenance of the organism. Within a population, inability to maintain any one of these activities at moderately extreme temperatures may be as decisive to continued survival as more extreme temperatures are to immediate life. Donaldson and Foster (1941) reported that when sockeye salmon fingerlings were gradually raised from 17.2°C. to 25.6°C. over a period of a week they "refused to eat and the respiration rate became excessive." By lowering the temperature to 21.1°C. feeding commenced and the fish "were able to make a very slight increase in weight." Water temperatures in the range of 4°C. to 7°C. produced poor growth. At a temperature of approximately 10°C., growth and food utilization were highest for the temperature conditions studied.

Adult salmon move inshore and upriver when maturing and spawn in freshwater streams. In doing so they must pass through water temperatures which may be far more extreme than those under which they finally spawn. F. F. Fish (1948) reported that during the summer of 1941 "the temperature of the Columbia River at Bonneville averaged 68.5°F. (20.3°C.). On July 20, 1941, the Columbia reached an all-time high temperature of 74.5°F. (23.6°C.)—hot water indeed for the blueback salmon (*Oncorhynchus nerka*), but a congenial temperature for certain of their microscopic enemies. One of these, the myxobacterium *Chondrococcus columnaris* is particularly lethal to blueback salmon." The great losses in these salmon, almost obliterating the 1941 run, were attributed to the combined effects of high temperature and bacterial infection.

If the lethal temperature were directly related to other aspects of temperature and survival, in a like manner throughout species, it would constitute a very convenient index for a measure of the limitation of species. Lethal limits and the distribution of animals in temperature zones on the earth's surface would be in direct proportion, where temperature was the restricting factor. Frequently, such relations are but poorly represented. The crucial question appears to be what activity is possible in the region of the lethal temperature. If the ability to be active were not seriously limited at temperatures approaching the lethal level, such vital activities as feeding, competing, and migrating would still be within the powers of the animal up to that temperature. Slight increases in the temperature tolerance of the species would increase the possibility for greater environmental experience, and consequently be of survival value. Conversely, where the activity of the animal was restricted in the region of its lethal temperature, an increase in tolerance without an increase in the scope for activity would confer comparatively little survival value.

The work of Fry (1947a, b; 1948) and of Graham (1949) illustrates the points of this proposition. Fry (1947a) has reported the active and standard metabolism of the bullhead (*Ameiurus nebulosus*) at various temperatures ranging from 2°C. to 35°C., and bordering on the ultimate upper lethal

temperature for this species (about 37°C.). The difference between the active and standard metabolic rates at any given temperature is taken to be a measure of the scope for activity, the lower level of metabolism being ascribed to the energy required to maintain the animal when inactive. In this species the scope for activity increases with increasing temperature over the complete biokinetic range. The bullhead seems limited only by its ultimate upper lethal temperature rather than by any loss in available energy for activity. The speckled trout, however, reaches a peak scope for activity at about 19°C. (Fry, 1947b; Graham, 1949). Increasing temperature above this level results in decreasing ability to be active, down to a relatively low scope when approaching the ultimate upper lethal temperature. The bullhead is quite understandably a successful fish in the high temperatures of shallow lakes and inshore reed beds, whereas the speckled trout, although capable of withstanding a temperature as high as 25°C., is not likely to succeed in this environment.

Activity has also been measured by the speed at which fish can swim steadily in a rotating chamber (Fry and Hart, 1948). A relation between this measure of activity and that from a difference in standard and active metabolic rates was established for the goldfish (Fry, 1947a; Fry and Hart, 1948). A most appropriate work in this field on salmonoids was that reported by Fry (1948) for two chars, *Salvelinus namaycush* and *S. fontinalis*, and two trouts *Salmo gairdneri* and *S. trutta*. Yearling fish of the various species were compared with respect to scope for activity by both the above methods at temperatures ranging from 5°C. to 25°C. Fry (1948) commented that "although it is not certain that the difference between trouts and chars has a phylogenetic basis, these two groups show a striking difference in their *swimming-rate* versus *temperature* curves." The trouts both appeared to be capable of increasing activity up to their ultimate upper lethal temperatures, whereas the chars showed a reduction in activity as the temperature approached the lethal level.

If this relation of activity to temperature has survival value, cases in which similar or nearly similar lethal temperatures exist, accompanied by dissimilar peaks for maximum activity, might be expected to have different distributions according to temperature, under otherwise relatively

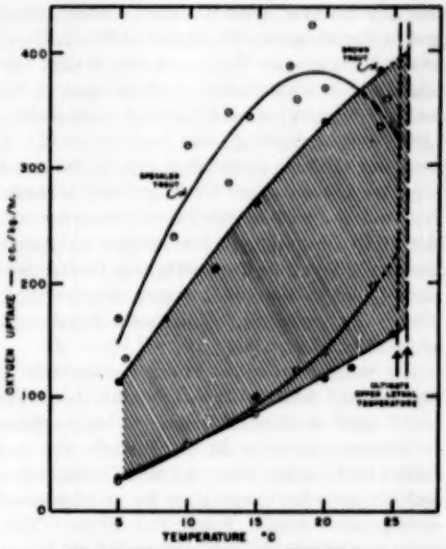

FIG. 3. THE RELATION BETWEEN TEMPERATURE AND METABOLISM FOR TWO SALMONIDS

The metabolism for speckled trout, *Salvelinus fontinalis*, and brown trout, *Salmo trutta*, has been measured as the active (upper) and standard (lower) levels of oxygen uptake when previously acclimated to each given temperature. (Data from Fry, 1948; Graham, 1949).

similar habitat conditions. The case of the brown trout and speckled trout appears to fall in this category (Fig. 3). King (1943) recorded that the brook or speckled trout is "considered a 'headwater' form and usually occurs in the upper stretches of trout streams," whereas the brown trout occupies "a position in the stream as a rule below that of the brook trout, but usually with an overlapping of ranges." Such a relation is supported by the work of Holton (1953). On the basis of their temperature-activity relations this is what might be expected. The brown trout shows a greater scope for activity at higher temperatures than does the speckled trout. The apparent support which these observations provide for the activity hypothesis cannot be pressed in any more serious form. The complexity of the problem may be judged by the diversity of opinions on trout distribution expressed in the literature which King (1943) has summarized most appropriately.

PREFERRED TEMPERATURES

The congregation of fish within a segment of the thermal range available to them has been re-

peatedly observed, both in nature (Dendy, 1945) and in the laboratory (Doudoroff, 1938; Sullivan, 1949; Sullivan and Fisher, 1953). Within the laboratory it is possible to eliminate many of the multiple factors of the natural environment (gradients of light, of gas and chemical content, of food and predator abundance) and to determine a *preferred temperature*. Like the lethal temperature and the temperature for optimum activity, the preferred temperature is subject to change through acclimation. Fry (1947a) has further indicated that a *final preferendum* may be established, where the preferred temperature equals the acclimation temperature.

The recognition of an ability among fish to perceive and select a limited thermal range has found significance in its relation to temperatures for optimum activity. In the goldfish, Fry and Hart (1948) noted that the final preferendum and the optimum temperature for cruising speed corresponded closely. Fisher and Elson (1950), using an electrical stimulus, measured the length of the initial dart made by speckled trout (*Salvelinus fontinalis*) and Atlantic salmon (*Salmo salar*) parr at various temperatures. The maximum response occurred in the region of the preferred temperature, for the level of temperature acclimation of their samples.

Whatever the mechanism or process of temperature selection may be, the survival value of moving progressively into thermal conditions which offer a maximum expression for activity is most apparent. It is interesting to note that speckled trout are not usually found at temperatures exceeding 19°C., which is the upper limit of their preferred range, although considerably below their upper lethal level.

Doudoroff (1938) concluded from experiments with the marine fish *Girella nigricans* that the temperatures most frequently selected (26–27°C.) were not in keeping with the temperatures of their normal habitat (mean surface temperature of 20.8°C.). This apparent discrepancy may be attributed in part to the phenomenon of summation in acclimation, previously referred to for the bullhead (Brett, 1944, 1946), in which the field collections showed an acclimation to the maximum rather than to the mean temperatures of the environment. From Doudoroff's data, the final preferendum lies between 23° and 24°C., in agreement with the highest mean monthly record of 23.6°C.

DIRECTIVE FACTOR

The laboratory study of preferred temperatures reveals a considerable degree of insensitivity to temperature. The range for a preferred temperature observed among young Pacific salmon frequently amounted to eight Centigrade degrees (Brett, 1952). This would hardly convince the ecologist that temperature could act as a directive factor for the relatively precise migrations of at least one of the species (*Oncorhynchus nerka*). Yet their exodus from lakes has been shown to correlate significantly with rising spring temperatures (Foerster, 1937). Kennedy (1940) discovered a temperature relation between the movement of lake trout from a shallow lake, as it warmed in spring, to a deeper adjoining lake. In Lake Oredon, Jammes (1931) observed that the vertical distribution of trout was related to temperature, their position being apparently defined by that of the 12°C. isotherm. In the ocean, the spring movements of Atlantic mackerel (Sette, 1950) "appear to be spawning migrations in which temperature limits the northerly progress" regardless of feeding conditions.

It is also relevant to note the delicate sense of temperature perception displayed by *Jenkinsia lamprotaenia*. Breder (1951) observed schools of this species to avoid consistently a temperature of 30°C., although moving freely in a temperature of 29.0 to 29.5°C. and capable of withstanding 35°C.

Using the method of conditioned responses, Bull (1936) was able to demonstrate particularly fine temperature discrimination in a wide variety of species. Responses were obtained for temperature increases as small as 0.03 to 0.10°C. A minimum difference of 0.5°C. was required to produce an unconditioned orientation response in two species of *Pomolobus*, the alewife and the glut herring (Collins, 1952).

Critical temperature conditions have been ascribed to maturation and spawning in some fish. Bullough (1939) discovered that in conjunction with long daily periods of artificial light a temperature of 17°C. brought on maturation of the gonads in the minnow *Phoxinus laevis*. A temperature of 17°C. alone was found sufficient to induce completion of spermatogenesis in the stickleback, *Gasterosteus aculeatus* (Courrier, 1922). Craig-Bennett (1930) further noted that in the stickleback a rise in temperature during the spring was sufficient to change "potential maturity" to "functional maturity." Similarly, a rise in temperature

in the presence of vegetation is reported by Fabricius (1950) to release spawning activities in the pike (*Esox*), whereas the whitefish (*Coregonus*) require a lowering of water temperature with a particular character of bottom.

Thus it would seem that an inherent ability to perceive fine gradients of temperature is only exercised when called upon by some internal drive (possibly in some migrations) or by an environmental stress (possibly at higher temperatures). This would constitute a distinction between temperature as a directive factor and temperature as a preferendum. The preferred temperature does not exert such a "pressure" eliciting particular activities, but has a wider range of effectiveness governing the efficiency of performance of the fish.

SUMMARY

Among aquatic gill-respiring vertebrates there is no escape from environmental temperature. Tissue temperature and environmental temperature are constantly equilibrated at the gill surfaces.

Although the thermal requirements for fish have been seen to fall in a number of categories, the fundamental requirement can be restated—an *external* temperature best suited to the *internal* tissues. The tissues themselves are functionally adapted to a wide thermal tolerance, which is bounded by lethal limits and shows great diversity between species.

In general, cold-water fishes have a lower level of thermal tolerance than inhabitants of warmer waters.

Through reversible acclimation, which is more rapid for a rising temperature than for a falling temperature, the degree of tolerance and the activity range is variously extended among species. A slow rate of *decrease* in environmental temperature is of greater importance for maintaining life than a slow rate of *increase*.

The combined information concerning lethal temperature and temperature permitting maximum activity, at all stages of development, is necessary for an interpretation of temperature in terms of survival value. Where high temperature does not limit activity, lethal levels can be expected to be a prominent environmental factor in setting the limits of distribution.

The scope for activity of cold-water fishes finds maximum expression at moderately low temperatures, which appear to be those frequented by the fish as preferred temperatures.

A potential for delicate discrimination of temperature differences among diverse species of fish is probably only exercised under particular environmental conditions.

Temperature, then, may act in the capacity of a lethal factor; it constantly conditions the fish through acclimation while governing the scope for metabolic rate; performance is best in the region of the preferred temperature; and a sensitivity to small gradients of temperature may act as a directive factor. Undoubtedly, the role of temperature among fish is a multiple one.

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THE STRUCTURAL AND FUNCTIONAL DIMENSIONS OF A HUMAN EXISTENCE

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IN VIEW OF the tremendous acceleration in our knowledge of man during the present century—biological, psychological, anthropological—it might be supposed a silly question to ask: What are the structural and functional dimensions of a human existence? Yet the late E. A. Hooton, in the introductory paper to a symposium presented in 1953 on *The Non-Human Primates and Human Evolution* (Hooton, 1955) began by saying, "Anthropology is the study of man, but difficulties have been encountered in formulating a scientific definition of man." At the same symposium C. R. Carpenter made the provocative statement that the assumption, although acceptable "either implicitly or explicitly" to many of his colleagues, "that the phenomena known as 'mind,' language, society, culture and 'values' exist exclusively on the level of human evolution" was untenable for him. What, then, *are* the distinguishing features of a human existence?

PROBLEMS OF CHARACTERIZING MAN

Two years prior to that symposium, K. P. Oakley, reflecting on the problem presented by the Australopithecines, published an essay entitled "A Definition of Man" (1951). He characterized attempts "to distinguish man from the rest of brute creation [by labeling him] the reasoning animal, the talking animal, the tool-making animal [as being] for the most part . . . philosophical exercises." In the end, however, he settled on man as the tool-maker as the best working definition. The persistence of this characterization ever since the pre-evolutionary days of the eighteenth century probably deserves independent investigation. It has been attributed to Benjamin Franklin, although no one seems to have cited an explicit reference in his voluminous writings (see note in White, 1942). In the twentieth century, Bergson's concept of *Homo faber* promoted the characterization in intellectual circles far beyond anthropology. Dr. Glass has called my attention to a reference in Sherrington and to the implications in Korzybski's "time-binding" concept in regard to the idea that a tool-making animal is one who anticipates the future (for an exposition of Korzybski's ideas, see Montagu, 1953). Among contemporary anthropologists the remarks of Le Gros Clark (1953) and Straus (1953b; 1955) are sufficiently representative of the use of this characterization.

But Oakley did not imply by his definition what others who have used the same label before him have implied. While he asserted tool-making to be a *human* trait, it is not characteristic of the family Hominidae as a whole, but only of later members of this family, in particular, of the genus *Homo*. Oakley has elaborated this point elsewhere (1953b) by remarking that "in the discussion on the evolution of man, one should keep quite clear these distinctions between the Hominidae and man. . . . The ancestors of early Pleistocene man, such as *Pithecanthropus*, were below the level which we take to be human from the point of view of brain size or from the point of view of function, that is, tool-making, and yet one would not say, 'Well, now, those are another family, those are Pongidae.' Whether one should have some subgeneric subfamilial name such as 'Prehominidae,' I do not know, but it seems to me it is very important to keep that idea in mind." All this is reminiscent of French anthropological writing, where we often find a "rigid line of specific distinction between *Homo faber*, that catchall of presumably soulless though manually dexterous super-anthropoids or 'prehominiens,' and *Homo sapiens* who is man" (Briggs, 1949). In this connection it is interesting to note that J. Huxley (1953) has used the term "proto-men" to distinguish early hominid types such as Java man and Peking man from "man in the proper sense of the word," that is, *Homo sapiens*, representing the "fully human phase." W. W. Howells has also pointed out (1950) that "we

have reached the point where such terms, especially 'human,' 'ape,' 'anthropoid' and 'man-ape,' are actually embarassing, except as applied to living forms, or in the case of the last, to the South African australopiths. When we talk about anthropoid and human history, the word 'ape' keeps changing its meaning." Even more recently Le Gros Clark (1955) has warned us against the colloquial usage of such terms as "man" and "human," since they are not synonymous with "the zoological terms Homo and Hominidae or to the adjectival form hominid." There is no doubt about the ambiguity of our English terminology.

The problem that has arisen seems to focus upon the difficulty, when dealing with possible transitional organic forms, of coordinating structural and functional data in a unified evolutionary picture. Some investigators, like Zuckerman (1954), have used both structural and functional criteria as a matter of course. According to him, "there is no difficulty about defining the major overt features in which man differs from all other Old World or catarrhine Primates. Man is a big-brained Primate with the power of articulate speech. He walks erect and uses his hands—emancipated from the task of carrying his body—to work with artificially fashioned tools. His teeth, and correspondingly his face, are small relative to those of the apes, and he is omnivorous as opposed to being predominantly frugivorous. These are the essential physical qualities [sic] which distinguish him from the ape, whose capacity for speech is confined to the utterance of sounds the connotation of which is purely emotive and whose very limited ability to use 'permanent tools' as demonstrated in experimental studies, is probably never manifested in the wild." Perhaps Zuckerman has less difficulty in distinguishing man from other Primates because he is one of those who have concluded "that the Australopithecines were predominantly ape-like and not man-like creatures."

It is interesting, then, to note the emphasis given in current discussion to functional rather than structural criteria. [For a contrasting non-functional emphasis see Dobzhansky (1944) and Mayr (1950).] With respect to the question whether the Australopithecines should be grouped with the Hominidae or the Pongidae, Le Gros Clark (1950) says, "Taxonomic difficulties of this sort, of course, are bound to arise as discoveries are made of fossils of a seemingly transitional type, and with the increasing perfection of the fossil record, probably the differentiation of man from ape will ultimately have to rest on a functional rather than an anatomic basis, the criterion of humanity being the ability to speak and make tools." And in a recent review of the evolutionary question Father Koppers (1952) asserts, "It is of course for the historians (prehistorians, social historians and ethnologists) to decide whether the Australopithecines are to be reckoned as human beings or not. Only when *culture elements*, objects produced by humanly intelligent activity, have been irrefutably established can the existing doubts and uncertainties be dispelled."

A similar stress on functional criteria is likewise reflected in the broad-gauged treatment of man's position in the evolutionary hierarchy presented by Julian Huxley and G. G. Simpson. Both of them have rigorously rejected the concept of man espoused by what Huxley has called the "nothing-but school"—"because man is an animal, a primate, and so on, he is *nothing but* an animal or *nothing but* an ape with a few extra tricks" (see Simpson, 1949). Simpson comments (1949), "It is important to realize that man is an animal, but it is even more important to realize that the essence of his unique nature lies precisely in those characteristics that are not shared with any other animal. His place in nature and its supreme significance to man are not defined by his animality but by his humanity." And Huxley (1953) says, "A greater flexibility of behavior, and a higher organization of awareness, enabled living substance to become capable of conceptual thought and symbolic language; and these . . . are the two distinguishing marks of man, and the basis of the latest deployment of life." What Simpson and Huxley are stressing are behavioral criteria that far transcend man's purely taxonomic status in the world of living things.

FUNCTIONAL AND STRUCTURAL EVOLUTION AND CULTURE

The use of behavioral or functional criteria such as speech or tools represents, of course, a piecemeal approximation to a categorical distinction which has been current in general anthropology for a long time. It is that man is unique among the Primates, and stands apart from all other animals as well, in possessing culture. In fact, the concept of culture and the identification of a human level of existence with a cultural mode of adaptation

seems to have become a commonplace not only among cultural anthropologists but among archeologists and physical anthropologists, to say nothing of sociologists and others. Not so long ago, Kroeber (1950), said that "the most significant accomplishment of anthropology in the first half of the twentieth century has been the extension and clarification of the concept of culture."

There are some indications, however, that it is not quite so easy to apply the generic concept of culture as the differential criterion of a human status, as was once thought to be the case. I have already pointed out that Carpenter, whose field researches among infra-human primate groups remain unparalleled, is not convinced that culture is a unique human phenomenon. The recent reactions of Straus to the application of the culture concept illustrate another facet of the same problem. In his remarks at the Wenner-Gren Symposium, Straus (1953) defined man as a primate possessing culture, and touched upon key points that cultural anthropologists have stressed for a long time. However, later when referring to his remarks at the Wenner-Gren conference, Straus (1955) said, "Subsequently, it has been amply demonstrated that the term 'culture,' even in its application to present-day human groups, is used inconsistently by anthropologists and without general agreement as to its precise meaning.... The term 'culture' remains particularly 'vague' and 'virtually undefinable' when applied to the comparative study of behavior within the order Primates." What Straus almost suddenly seemed to become aware of was the more acute and sophisticated analyses of the meaning and conception of culture which have recently come to the fore in anthropology; in particular, the monograph by Kroeber and Kluckhohm (1952), which offered 164 definitions of culture systematically culled from the literature of Europe and the United States. Straus referred, among other items, to a review of this monograph by Leslie White (1954) in which the latter wrote, "Thirty years ago most anthropologists—in the United States at least—knew what they meant by culture, and most of them meant, I believe, what Tylor meant in 1871 when he formulated his 'classic' definition. ['Culture or Civilization, taken in its wide ethnographic sense, is that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society.'] But who knows what Mr. X means by culture today? Culture is 'learned behavior'; it is not behavior at all but 'an abstraction from behavior'; it is 'intangible,' a 'logical construct'; it is a 'psychic defense system'; a 'precipitate of social interaction,' a stream of ideas; it 'consists of *n* different social signals that are correlated with *m* social responses,' etc. One anthropologist at least has gone so far as to question the 'reality' of culture."

To anyone who has not lived through some of these discussions the situation may well seem more chaotic than it actually is. Where, then, do we stand with respect to "culture" as the primary differential between man and other Primates? Rather than seek directly for a formal definition, I prefer to phrase the question as, What can be said about the necessary and sufficient conditions that distinguish a human level of existence?

In the first place, I would note that culture as dealt with by cultural anthropologists has been observed, analyzed, conceptualized, and defined exclusively on the basis of data derived from ethnography and history. In the second place, direct observations and historical records apply to only one species of man—*Homo sapiens*. For other members of the genus *Homo*, behavior must be inferred from archeological material. In other words, culture in its richest and, to the cultural anthropologist, most typical forms—running the gamut from speech through technological equipment and its material products, to social and economic organization, music, and world view—is associated with *Homo sapiens* alone, so far as reliable observations go. Indeed, it has been the common denominators of culture and the over-all similarities in basic pattern which, together with species identity, have provided the best argument for the world-wide comparability of cultural data as well as the generic concept of culture.

Consequently, it is only by inference that we can possibly extend this whole complex to other groups of the Hominidae, a zoological family which was certainly not differentiated in the first place by the possession of culture. For those who include the Australopithecines as a subfamily, bipedal locomotion becomes the unifying trait of the whole taxonomic group. It is easy to see the pertinence of Oakley's comment (1953) that "when you try to translate 'man' and 'human' into scientific terms you get into difficulties." Thus if the Australopithecines are classified as Hominidae on structural grounds, this does not make them "human." And since it is not yet established to the satisfaction of everyone that the australopiths

made tools, and no one has seriously argued that they possessed speech, the old equation man = human = culture cannot be correlated with the zoological category Hominidae without further analysis. Then there is the general theory, now widely current, that modification in posture was the first and most important step in human evolution which preceded the later expansion of the brain in the Pleistocene and was necessary in order to free the hands for tool-using (see, e.g., Mayr, 1950, and particularly Washburn, 1950).

If there were differential structural lines of development taking place during the Pleistocene, how are we to conceive the accompanying behavioral stages with reference to culture? The structural changes that made possible erect posture and bipedalism provided the initial foundation for a novel behavioral plateau. But, however the behavioral patterns of this stage in hominid development may be conceived, it is surely impossible to identify them with those that must have developed later as a consequence of an expanded cortex. Our concept of culture, it seems to me, must now take account of differential behavioral plateaus which, in turn, are related to structural changes.

Are we to assume that the early Pleistocene hominids Coon (1954) has labeled "half-brained men" possessed culture in the same sense as *Homo sapiens*? Is a fully developed human brain structure irrelevant as a prerequisite for a fully developed cultural mode of adaptation? Is a "half brain" as good an instrument as a whole brain for the development and functioning of human speech, music, the graphic and plastic arts, abstract thinking, and religion?

This leads to a third observation. Along with their rejection of the notion of unilinear stages in cultural evolution, twentieth century anthropologists have implicitly, if not explicitly, conceived of culture as some kind of whole with a relatively constant categorical content, in so far as it constituted a human differential. Disparate chronological relations between various cultural traits—such as fire in relation to tools, speech in relation to fire—have, for the most part, been avoided. This has led to a somewhat paradoxical situation: whereas opponents of human evolution in the nineteenth century were those who naturally stressed evidence that implied discontinuity between man and his primate precursors, anthropologists in the twentieth century, while still giving lip service to morphological evolution, have, by the special emphasis laid upon culture as the prime human differential, implied what is, in effect, an unbridged behavioral gap between ourselves and our closest relatives. The possession of culture has tended to become an all-or-none proposition.

The principal theory which exemplifies this point of view in American anthropology is the one advanced by Wissler in *Man and Culture* (1923). Wissler projected a "universal pattern" of culture full-fledged from properties he conceived the human germ plasm to possess. He said, "the pattern for culture is just as deeply buried in the germ plasm of man as the bee pattern in the bee," so that "a human being comes into the world with a set, or bias, to socialization, according to a definite pattern . . . by reason of which a man is a human being and not a termite, a bee or even a monkey. The human pattern, therefore, is a part, if not the whole, of man's inborn behavior . . . man builds cultures because he cannot help it, there is a *drive* in his protoplasm that carries him forward even against his will." This is psycho-biological determinism with a vengeance. Actually, the content categories of Wissler's universal pattern paralleled very closely the chapter headings of any standard ethnographic monograph: technology, social organization, myths and tales, religion, and so on. But Wissler did not elaborate his hypothesis for the different genera and species of fossil Hominidae. He did not say whether the universal pattern for culture was embedded in the germ plasm of *Pithecanthropus* as well as *Homo*, and *Australopithecus* had not yet been discovered. Even later writers, like Murdock (1945), who, following Wisler, have made use of the idea of a "universal pattern" which "links all human cultures, simple and complex, ancient and modern" and whose origin must therefore be sought "in the fundamental biological and psychological nature of man and in the universal conditions of human existence," do not specify what "human" refers to in a biological frame of reference. As a matter of fact, it has always remained an open question whether the generalizations that have been made about "man" apply only to the genus *Homo* or more widely; but the lack of empirical data, linguistic as well as cultural, has left the answer moldering in limbo.

We have become more critical, too, particularly of various possible inferences from structure to function. It was not so long ago, for example, that deductions regarding capacities for learning, manual skill, and talking inferred from the endocranial casts of early genera of the Hominidae, were taken

more seriously than now (Le Gros Clark, 1955). In these latter days, however, one hears repeated Weidenreich's classic statement that the interpretation in detail of an endocranial cast is "no more reliable than any other form of phrenology." Speech was one of the major items in Wissler's universal culture pattern, but can we now assert with any confidence that speech was characteristic of all the Hominidae? If the Australopithecines are included in this family, the unequivocal answer would be No! And even if we don't, there is now no unanimity of opinion.

It is sometimes worth while to recall past speculation when considering contemporary problems, and in connection with the question of speech as a generic "human" trait, it will be remembered that Lord Monboddo and others in the eighteenth century thought that speech was *not* typical of the earliest stages of human development. Within the past few years, two English biologists, R. J. Pumphrey and J. B. S. Haldane, have also argued that human language is a very, *very* recent development. While communication is not denied—since varying forms of animal communication have been described—what Haldane (1955) calls "descriptive language" only appeared "with the technological revolution of the Upper Paleolithic." Pumphrey (1953) says that "on the grounds that the use of tools and the use of speech imply intellectual processes of the same order which are characteristically human, some authorities have inclined to refer the origin of speech to the lower Pleistocene or even to the Pliocene." The author attempts to refute this idea. Whatever we may think of his arguments, it is a striking fact that he is not alone in depriving several early types of man of a capacity most anthropologists were once inclined to attribute to them. Following this line of reasoning, there would have been "culture" long before speech (in the sense in which we are familiar with it in *Homo sapiens*) developed.

There is likewise a logical parallel between the way in which culture, in the generic sense, has been conceived as a constant pattern of categorized items, typical of man from the very beginning, and the assemblage of physical traits (erect posture, big brain, reduced canines, and so on) which biologists have used so often to characterize a human status. Both sets of patterned traits could be fitted together with little difficulty so long as the temporal aspects of the chronological picture remained somewhat vague, the actual gaps in man's ancestry remained unfilled, and so long as fossil forms that might suggest transitional structural and behavioral stages did not turn up. Until this happened, a fair-sized brain and erect posture, with speech inferred, could remain associated as indications of an unequivocal human status; and if tools and the use of fire could be demonstrated besides, this behavioral evidence clinched the matter because "culture" could then be inferred. Lip service could be given to evolution but many of the complexities of the process could be sidestepped, such as the evolution of different physical characters at different rates, as well as different behavioral patterns and culture traits. As Howells (1950) has pointed out, "Heretofore we have been given to talking about 'the appearance of man'—the tyranny of terminology—as if he had suddenly been promoted from colonel to brigadier general, and had a date of rank. It is now evident that the first hominids were small-brained, newly bipedal, protoaustralopith hominoids, and that what we have always meant by 'man' represents later forms of this group with secondary adaptations in the direction of large brains and modified skeletons of the same form." Analogically, it is equally doubtful whether we should any longer talk in terms of the "appearance of culture," as if culture, too, along with man had almost suddenly leaped into existence. For if we do, we may unwittingly find ourselves in the *anti-evolutionist* camp. Only recently Father Koppers (1952) has forcibly asserted: "The ethnologist when surveying his field, no matter how far removed in space or in time, finds only man complete with understanding and freewill and no trace of wholly or half-animal pre-human beings. In the fact of ethnological data, it is impossible to uphold the extreme principle of evolution. We call it an extreme point of view, in so far as it has not confined itself to organic matter but has sought from time to time—and still frequently seeks—to include the field of mental development within its orbit."

But once we have adopted an evolutionary frame of reference, how is it possible to draw an arbitrary line between morphological evolution and behavioral evolution, between structure and function? On the functional side, even though not open to direct observation, we must assume a social and psychological dimension. The infra-human primates maintain a social existence, and the later level of adaptation we have called cultural cannot be considered apart from a psychological dimension in evolution except by *deliberately* excluding it. As a matter of fact, comparative psychology

has made it its business to deal with the psychological dimension of evolution, starting far below the primate level. And learning theory as applied to man has been developed to a considerable degree through experimental observations on rats!

The problem of human evolution, then, is inherently complex, since variables other than organic ones must be taken into account. We are compelled to recognize a novel adaptive plateau where the actual determinants of behavior are less and less relevant to innately acquired structures. Once we begin to consider all these complex variables in relation to one another, to weigh them and order them chronologically as best we can, the nearer we shall be able to approach an answer to the basic question: What are the necessary and sufficient conditions that have made a human existence possible?

PROTOCULTURE AND CULTURE

I believe it can be shown that among the necessary, but not sufficient, conditions prerequisite for the development of a cultural mode of adaptation are those which must have linked the early Hominidae with their precursors and with other primate forms as well. Furthermore, I think we need a term to characterize an intermediate phase or plateau of behavioral evolution which carries some, but not all, of the connotations with which the concept of culture has been invested. The term I suggest is *protocultural*.

Data on infra-human primates observed in their natural habitat or under laboratory conditions suggest some lineaments of this phase. For deductions from comparative behavior are as methodologically legitimate as those from comparative anatomy. It seems to me, too, that the concept of protoculture may prove useful in resolving the question: do the infra-human primates have culture? The answer to this question too often has depended upon a formal definition of culture that takes no account of the evolutionary process. At the same time the concept of protoculture should lend support to the traditional view that culture in its fully developed form *came to be* a unique "human" attribute, if human is not identified with hominid. Yet it does not leave this characteristic feature of human adaptation rootless when viewed in the perspective of behavioral evolution.

Let us now consider some of the key points involved in the differentiation I am proposing between a protocultural and a cultural level of adaptation.

1. The term culture, however defined, cannot be dissociated from the learning process. From the time of Tylor—who used the word "acquired"—culture has always been conceived as referring to learned behavior or the products of learned behavior.

2. While culture could only have emerged in a species capable of learning, the latter mode of adaptation is, of course, not a unique human phenomenon. It can be assumed as a capacity of most animals, whether primate or not. The more important question turns upon the role which learning plays, both quantitatively and qualitatively, in the total life history of the organism. In the evolution of the Hominidae in particular, there is the basic question, What is the relation between learning in the most advanced type of *Homo* and the expansion of the cerebral cortex?

3. Culture always has a locus in social groups —communities, societies—that are organized or structured units. The functioning of these groups is dependent upon social learning, i.e., upon influences mediated in social interaction. The individual has to be groomed to become an integral unit in an on-going social system.

Infra-human primates live in structured social groups and learning is likewise one of the conditions relevant to the operation of these at a protocultural level. Bartholomew and Birdsell (1953) make the point that "even on the non-human level, population density may be controlled by behavioral factors, either genetic or learned," and that "territoriality and dominance relations, which are dependent on learned behavior, contribute to the determination of group relations and population density." Carpenter (1942) has explicitly stressed the necessity of socialization as a condition for the integrative functioning of such groups: "A given number of monkeys and apes does not make or equal what I have been calling a group. . . . Suppose we try this experiment: Raise in isolation animals of the species, but of the right sex and age to compose a group which meets the requirements of the formula for the average group characteristic of a species. These individuals will then be released together. What will happen? Some may so fear others that they flee. Some will be antagonistic and fight. Others will form into groups and remain together, as had been hoped or predicted, into a single organization. Why? Even though social drives are operative and social incentives are present, the monkeys have not been conditioned to each other. They have not been socialized—i.e.

they have not learned to make fitting responses to each other as complexes of stimuli. What is lacking is what I have called *integration*.

"Social integration is conceived to begin with birth and to involve definable processes of social learning and adjustment. These processes are organic and involve the expressions and satisfactions of physiological drives. From one viewpoint, effective social integration of an individual conditions it in a manner to make it responsive to the communicative acts, motor expressions, including gestures, and vocalizations. These communicative acts, involving specific stimuli patterns and fitting responses, constitute the core of group coordination. Let it be remembered that the stimulus aspects of communicative acts cannot be operative except on a background of social integrations—i.e. animals which are conditioned to each other."

Tylor in 1871 defined culture as the "capabilities and habits acquired by man as a member of society." Hart (1938) has expressed the opinion that this definition "more than any other single sentence [is] responsible for the present gap between the biological and social studies. . . . Is there any real evidence [he asks] that man is the only species which can possibly profit by living in a society and thus acquiring capabilities and habits which aid his survival as a species? that methods of behavior learned after birth from the fellow members of the same species do not exist at all at any other level of culture, except the human level? If such evidence did exist, it would destroy entirely the view of nature as one continuous chain; the gap between organic and superorganic evolution would be as great as that between inorganic and organic, and astronomy would be as relevant to anthropological studies as biology." Hart goes on to say that in his opinion observations such as those reported by Zuckerman (1932) in *The Social Life of Monkeys and Apes* are "strongly indicative of the fact that in social life, as in morphology, the gap between the higher apes and man is small if not smaller than Huxley suspected and as few writers since Huxley have been willing to admit." The absence of references in the anthropological literature to Hart's article is itself indicative of a failure to come to grips with evolutionary theory in sociocultural and psychological, as well as morphological, terms. Today we can say that at the infrahuman level the monkey or ape also acquires habits as a member of society. Besides this, the functioning of these subhuman societies as integral units appears to be dependent to a large degree upon learning.

This relation between learning and social structure is continuously operative from a protocultural level up through a cultural level of adaptation. Besides sharing a gregarious existence with the infra-human primates, man also shares one of the basic mechanisms through which social life is structured at a lower level no less than at a higher one. Thus one of the basic conditions necessary for the expansion and elaboration of social behavior and group organization at a cultural level was established prior to the morphological changes that led to both erect posture and the expansion of the cortex. We can generalize even further and say that among the higher primates, even in the absence of speech, various habits may become socialized and transmitted as group attributes. For example, when the primate colony at Orange Park was established "the pioneer chimpanzees were shown how to work the drinking fountain, and through the years ape has aped ape and no further instruction of new generations has been necessary" (Howells, 1954). Another example would be nest-building among chimpanzees. Nissen (1955) has pointed out that there is good evidence that nest-building is not instinctive like grooming, "but is rather transmitted by imitation or tuition from one generation to the next: "it is, therefore, one of the very few items of behavior seen in these animals which may be classified as cultural." It might better be called *protocultural*. Culture involves much more than the social transmission through imitative learning of acquired habits (that is, the "social heredity" concept). Together with learning, however, and in the absence of speech, it may be considered one of the prerequisites of culture, perhaps the most important earmark of a protocultural level and a necessary condition for a cultural mode of life.

Eiseley (1955a, b) has called our attention to a most important fact in the course of man's development: so far as organic inheritance is concerned, man's gut is not that of a true meat-eater, nevertheless at a very early stage he became carnivorous. In other words, as a bipedal ground-dweller of the grasslands he underwent "a transition in food habits which is unique on the planet" (1955a). Linked with the discovery and the use of fire, the development of these new food habits in the Hominidae could only have become socialized and perpetuated through learning. If it be assumed

that this occurred because these dietary habits had a high adaptive value and that in the beginning they were acquired and socially transmitted without speech, this would be an example of one of the earliest stages in man's protocultural development.

James Boswell thought Franklin's definition of man as a tool-making animal was excellent because "no animal but man makes a thing, by means of which he can make another thing." But he ventures to offer a definition of his own: "Man is a 'cooking animal.' The beasts have memory, judgment, and all the faculties and passions of our mind, in a certain degree, but no beast is a cook." (Hill, ed., 1950.)

4. Beach (1949) has stressed the fact that the new feature of mating behavior in the primates is the "emancipation" of the female from the cyclic control of the oestrous hormones. "In no other mammalian group does such continuous mating provocation occur, for all other combinations of reproductive features lead to the limitation of mating behavior to certain physiologically defined periods, even when the animals come together in groups" (Chance and Mead, 1953). This suggests one of the underlying conditions that has led to the widespread occurrence of the biparental family among the infra-human primates, as contrasted with uniparental family groups among the lower mammals. Males, that is, became permanent members of a social unit consisting of the females with which they mate and their offspring. This over-all pattern of association seems to remain constant irrespective of variant forms (monogamous, polygynous, or sexually communistic as among the howlers).

In addition, infra-human primate societies are not only relatively small in numbers; only two generations are associated. Furthermore, individuals of the younger generation may leave at puberty and form other groups. This very limited temporal association between generations, combined with the absence of symbolic communication, imposes an inherent limitation upon the possibility that the transmission and accumulation of whatever learned behavior may exist—even the simplest habits—can affect the behavior of subsequent generations in any but very limited ways. Furthermore, two of the tentative generalizations which Carpenter (1955) has formulated on grouping behavior among non-human primates emphasize (a) that such groups "tend strongly to be autonomous, self-maintaining and regulating" and "express resistance and hostility to other organized groups of the same . . . species"; and (b) that "organized groups of a population of the same species in a limited region do not have super-group social mechanisms." There is thus nothing comparable with communities or tribes on the human level, and Carpenter adds, "Kinship relations are not operative, and inbreeding is the rule rather than the exception. . . ." Biparental family patterns are, then, another identifiable trait of a protocultural level which link the Hominidae with lower primate forms.

A cultural level is reached only when the biparental unit is *transcended* as the sole matrix of the social relations of individuals, and when the expanding dimensions of the social order to which individuals must adjust functions as a *moral order* as well. This development, in turn, is contingent upon the emergence of a novel psychological structure. [For a further elaboration of these points see Hallowell (1955), particularly Chap. 1, "Personality Structure and the Evolution of Man," and Chap. 4, "The Self and Its Behavioral Environment."] Sexual behavior now is evaluated in relation to traditionally recognized norms of conduct in a non-familial as well as a familial context. Social sanctions reinforce culturally constituted values and standards of conduct. The introception of moral values, unconsciously as well as consciously, has become a major determinant in the behavior of individuals with a *human* psyche.

5. Although so complex a topic is beyond the scope of this paper, it is necessary to say something about the study of communication in animals compared to man. As Hebb and Thompson (1954) observe, "One essential key to social organization is the means of communication, in the broad sense of the way in which the behavior of one animal induces cooperative behavior in another." These writers believe that "too much attention . . . has been given to man's special vocal behavior." In their broadly gauged conception of communication they include "reflexive" or nonpurposive communication as contrasted with phenomena of a psychologically higher order. In a recent bibliographical survey, Schneirla (1952) concludes, "An adequately comparative program of study of such phenomena is long overdue, particularly to clarify the relationships of concepts such as 'sign,' 'signal,' and 'symbol,' as well as the criteria of 'language,' all of which appear to suffer from a heavy load of speculation and a minimum of systematic research. Research on questions concerning levels of phenotypic relationships through successive generations

of lower animal groups, certainly basic to a needed re-evaluation of the broad problem of 'culture,' should enlist the active attention of social psychologists, sociologists and anthropologists alike." Since we do not have the results of such a program available, it is necessary to get along as best we can.

In the first place, I would assume that in all social animals some kind of communication takes place, although the sensory modalities involved may differ greatly. I would also assume that communication has a common generic function whether in animals or man: it is one means by which the coordination of behavior may be facilitated.

We know, of course, that infra-human primates lack speech, although they live in structured societies. We also know that a system of orally produced signs or signals is important in one group of New World monkeys that has been closely observed and in the Old World gibbons. Are the dozen distinguishable sounds produced by the howlers and almost as many produced by the gibbons identical throughout the species and innately determined? Or is learning involved?

If, in any non-human primate species, oral *signals* were learned and transmitted, and showed intraspecific variability, this would be a protocultural fact of great significance. As against this possibility, in the case of the howlers on Barro Colorado Island, Carpenter stresses group autonomy and hostility, but a common series of oral signals. This suggests that learning is not involved. In any event, these particular sounds seem to be clearly in the category of signals. Their utterance calls for action in some particular situation. They have no representative functions. They are not vehicles for the conception of objects or events (Langer, 1942). Even the chimpanzee who has been tested in the laboratory has not convinced observers that he has much capacity for symbolization, to say nothing of symbolic communication (Yerkes, 1943). And the observations on the home-bred Viki (C. Hayes, 1951) make it more evident than ever before that a chimpanzee, whatever its other accomplishments, cannot be taught to speak even with maximum encouragement. On the other hand, the Hayes seemed to experience no difficulty in communicating with Viki at a sublinguistic level. This was essential to the mutual social adjustment of Dr. and Mrs. Hayes and Viki, whose role was that of a child in their home. While Viki made relatively "little use of gestures with the hand alone, without contacting an object or person," she often pointed to things she wanted, and sometimes she made use of what the Hayes called "iconic signs." An example of the latter is when "she moved her empty hand back and forth above the ironing board, apparently to show what she wanted." Personally I should call this a gestural symbol, but this only shows the terminological difficulty *we* sometimes experience in communicating about communication. The Hayes go on to report that Viki could convert such signs into "symbols" (in the narrow sense), that is, could convey a meaning through a representative act that bore an essentially arbitrary relationship to its physical character. "When Viki was very young, we never took her for a ride in the car without taking some spare diapers along. As a result, she invented the device of asking for a ride by bringing us a handful of diapers from the bathroom. Later, she no longer wore diapers, but there were still some in the bathroom, and she still brought them out when she wanted a ride. When we eventually disposed of the non-functional diaper supply, Viki asked for a ride by running into the bathroom and coming out with a handful of Kleenex tissues—which only bore a faint resemblance to diapers. These tissues had never had any direct connection with rides, and by this time Viki had quite likely forgotten how the whole thing started. Except for its history, this would now appear to be communication by means of an arbitrary convention developed by the chimpanzee."

I mention this case because, in principle, this kind of symbolization developed further is exactly what we find in human speech where there is no ostensible connection between the sounds used and the object, concept, or event represented. If a mode of *gestural* communication based on such a principle had been observed functioning in a chimpanzee social group I would call it protocultural. But the only observation we have is on a single home-bred animal. Yet it is worth noting that we have here the *invention* of an arbitrary symbol motivated by the need to communicate. To become socially useful any non-iconic or arbitrary representation, whether vocal or not, must be learned by a series of individuals. This is one of the peculiar and distinctive features of human speech.

So far as communication in man is concerned, we know that the possession of speech presumes the capacity to invent and make use of extrinsic symbolic systems, that is, representation of objects and events that can be responded to not only by the organism itself but by other organisms to whom

the socially significant symbol is communicated and is the vehicle of meanings. Thus skill in the manipulation of symbols is directly involved with the development of man's rational capacities. But symbolization is likewise involved with all other psychic functions—attention, perception, memory, dreams, imagination, and so on. Representative processes, both intrinsic and extrinsic, are at the root of man's ability to deal with the abstract qualities of objects and events, the ideal as well as the actual, the intangible along with the tangible, the absent as well as the present object or event, with fantasy and with reality. A negative feature of the protocultural phase is the absence of any evidence that suggests the transformation of individual experience into any kind of socially significant symbols.

6. Finally, a word about man as the tool-maker and the status of tools at archeological horizons as an index to "culture." In recent years, with the emphasis given to the priority of erect posture over cortical expansion in human evolution, the use and making of tools in the Hominidae have been put in a new perspective. Bartholomew and Birdsell (1953) have gone so far as to suggest that "protohominids were dependent on the use of tools for survival." If this be true, then of course *tool-using* would long antedate speech, to say nothing of a cultural level of adaptation. But at a subcultural level quite a few animals use tools. This has been observed in birds (Darwin's finch); in infra-human primates, in particular, various species have shown considerable facility in implementation. The Pongidae do not appear to be especially superior.

Animal psychologists, however, have not found it easy to say exactly what constitutes tool-using. Liberally interpreted, it might well include the piling up of boxes in order to secure food, the use of sticks to achieve the same end, or pole vaulting. Nissen (1946) says, "Perhaps it should refer to performances such as the breaking off of a branch of a tree or the freeing of an iron bar, as described by Köhler for chimpanzees, in order to obtain an instrument for further purposes. The nearest thing to the manufacture of tools in the ordinary sense seen in primates, is the observation by Köhler of a chimpanzee fitting together two short sticks in order to make a long one." But this observation has not been repeated.

We may assume, then, that the chimpanzee and other infra-human primates have innate capacities that enable them to make occasional use of tools, and perhaps when highly motivated even to make them. But there is no evidence that would suggest more than this. We can, therefore, scarcely deny an equivalent capacity to the Hominidae. Possibly adaptation to a new environmental corridor made tool-using and fire, along with the acquisition of new food habits, of special survival value. However this may be, it seems possible that a protocultural stage in tool-using might have been reached rather early, even before the development of speech. This is suggested by the evidence of nest-building in chimpanzees. Thus, in greatly lengthening the perspective in which tool-using should be viewed, Bartholomew and Birdsell (1953) may be right: ". . . in contrast to all other mammals, the larger arboreal primates are, in a sense, tool-users in their locomotion. As they move through the maze of the tree tops, their use of branches anticipates the use of tools in that they routinely employ levers and angular momentum. The grasping hands on which the locomotion and feeding of primates depends are, of course, obviously preadapted for tool use. Rather than to say that man is unique in being the 'tool-using' animal, it is more accurate to say that ***man is the only mammal which is continuously dependent on tools for survival*** [my italics]. This dependence on the learned use of tools indicates a movement into a previously unexplored dimension of behavior, and this movement accompanied the advent of bipedalism. With the assumption of erect posture, regular use of tools became obligatory; the ability occasionally to use tools must have preceded this in time." Oakley's statement (1954) that "though man's Pliocene ancestors were not tool-makers, they were tool-users," appears to support this view. As empirical evidence Oakley refers to the baboon skulls at Taung, which presumably were pierced by artefacts in the hands of the Australopithecines.

Oakley goes on to say, "What is in doubt is when and why in their evolutionary career the Hominidae became tool-makers." Reflecting on this problem, he opens several interesting lines of speculation. He does not think it necessary to assume that the earliest hominid tool-users, or even tool-makers, possessed speech: "There are indications that speech, as we know it, though not necessarily language, was invented only at a comparatively late stage of cultural development." (The use of the term "cultural" in this statement is an example of the need for a more discriminating terminology and the recognition of developmental levels.) Oakley speculates further that "man's earliest means of communicating ideas was by gestures with the

hands . . . [and that perhaps] an increasing preoccupation of the hands with the making and using of tools could have led to the change of manual to oral gesturing as a means of communication." I am not concerned here with the plausibility of this theory; but since speech has always been considered an integral part of culture, any gestural stage in the development of communication in the Hominidae would clearly be at the level I have called protocultural.

On the basis of the empirical evidence now available, Oakley, in the same paper, presented a tabulation of six stages in the development of tool-using and tool-making in the Hominidae. The first stage is labeled "occasional use of improvised tools and weapons," and refers specifically to *Australopithecus* and Pliocene hominids. The second stage is that of "occasional tool-making" and the "dawn of Early or Lower Paleolithic." (It should be recalled here that it may be no accident of discovery that the remains of *Pithecanthropus* with which tools have been found associated [Peking Man] are of Middle Pleistocene dating.) "Regular tool-making with marked standardization" does not appear until the fourth stage, at which point the precursors of *Homo sapiens* are definitely involved. The tabulation is captioned: "Six Levels of Culture on the Basis of the Use and Making of Tools." Now it is interesting to observe that although Oakley has defined man as the tool-maker, yet he does not identify man with the Hominidae and denies to the early Hominidae the capacity for speech; nevertheless he converts "culture" into an umbrella term which, when fully expanded, he uses to cover every manifestation of tool-using and tool-making throughout the family *Hominidae*. It seems to me that without drawing too fine a line, the first two stages of Oakley's scheme would be clarified by calling them *protocultural* stages.

That some such line needs to be drawn is clearly indicated in the earlier part of his article by Oakley himself. There he stresses the wide psychological gap between *tool-using* by primates and *tool-making* by man. Referring to the chimpanzee Sultan who *made* a tool, Oakley points out that this feat was accomplished with a visible reward as incentive: "There is no indication that apes can conceive the usefulness of shaping an object for use in an imagined eventuality." He then goes on to cite a famous passage in Köhler's *Mentality of Apes* in which Köhler stresses the temporal limits, past and future, of the world of the chimpanzee. It seems to me also necessary to stress the enormous difference between the unique events in the life of Sultan that led him to make the famous tool and the human situation in which the material used for making tools, the forms they take, and the techniques of manufacture, are all part of a tool-making tradition which, in turn, is part of a more inclusive cultural whole. Sultan's feat, though remarkable, is at a subcultural level because he could not learn either tool-using or tool-making from his fellow-apes. On the other hand, so far as the early Hominidae are concerned, it seems possible, as I have said, that a tradition of *tool-using* for limited purposes and on certain occasions could have arisen before the development of speech. However, if the tools found associated with some of the early Hominidae are interpreted in this way, the level would be protocultural rather than cultural. I would assume that one of the criteria of this protocultural level might be the absence of standardization or functional differentiation in tool forms and little, if any, evidence of inventiveness and technological progress. The opposite would be the case where a full-blown cultural level had been reached.

A number of years ago Leslie White (1942) published a paper, "On the Use of Tools by Primates" (1942). The point he made has been overlooked in recent discussions of man as the tool-*maker*. His conclusion was that "on the material and mechanical side, as well as upon the intellectual and social, culture is dependent upon the use of symbols." Tool-making at the human level implies an act performed in the present which cannot be dissociated from a purposeful use of the object at some future time. In the absence, then, of some traditional means of symbolically mediated temporal orientation, how would such behavior be possible for an ape (or perhaps an early hominid)? Tool-making is *psychologically* much more complicated than tool-using. Among other things, we would have to assume that the ape possessed a capacity for "self-awareness," and that he could somehow represent himself in some future time to himself. Thus, even though Sultan made a tool, the psychological field in which he acted was qualitatively different from that in which man acts in his tool-making. The goal toward which Sultan's needs were directed was in his immediate perceptual field, and the reward he claimed was not long delayed. But a far more important point of difference is that a man does not just make a tool: quite apart from any technological knowledge involved, he *shapes* it, and for this

he must have some image in mind which necessitates intrinsic representative processes. Besides, the shape image he has is not usually idiosyncratic: it is related to the shape of the tools in his cultural tradition in the same way that the material used and the technological knowledge involved are related. It is difficult to see, then, how these factors could be integrated or transmitted without speech. Yet in Pumphrey's discussion of the relation of speech to tools he remarks that there is "no valid reason for assigning intellect to a maker of implements."

Thirty years ago Grace A. de Laguna (1927) argued that tool-making is integral with man's capacity for dealing indirectly with things through the functioning of analytical discrimination of their objective properties not only in relation to himself, but to other things. The tool-maker must be able to distinguish clearly among properties relative to the ends he has in view. This kind of perception transcends the infra-human primate level: it necessitates conceptual thought. Thus, "it is scarcely credible, even aside from the more theoretical psychological considerations, that the art of chipping stone implements could have been developed by men who had not yet learned to speak." Furthermore, to have undergone any great development, since "the evolution of tools is essentially a social evolution"—or as we would now say, cultural—a primary condition was "the permanent organization of the social group, bound together by language." In a later, unpublished manuscript de Laguna has succinctly expressed her thought by saying, "*Homer faber* is *Homo cogitans*."

Pumphrey seems to think that the spider's web and tools actually belong to the same category. If "man" be defined as a tool-maker rather than a tool-user, and artefacts in archeological horizons be interpreted as evidence of a "human" status, this characterization remains restricted, if not arbitrary, in significance unless the socio-psychological implications of tool-making as an integral part of the problem are fully clarified. A scientific definition of man, to be really inclusive in scope, is contingent upon the integration of all relevant data and upon an analysis that gives full weight to all the socio-psychological factors involved in a human existence.

My purpose in suggesting that we need to discriminate between different levels of adaptation in the behavioral evolution of man is based on the fact that structural criteria alone do not seem sufficient to define a human level of existence. The recognition of a protocultural stage takes cognizance of behavior linkages between the Hominidae and other primates which, although they may be difficult to deal with, are of no less theoretical importance than the more precisely determinable morphological facts.

SUMMARY

1. Many definitions of Man have been proposed, but due to limited knowledge or a unilateral disciplinary approach they have been restricted in scope and have not met with unqualified acceptance.

2. The problem has persisted even into a period when reliable knowledge about the chronological aspects of man's development has been accumulating at an unprecedented rate in archeology and physical anthropology; when novel behavioral observations on infra-human primates have become available; when the data of cultural anthropology have increased immensely, and psychologists have gained new insights into the determinants and mechanisms of behavior.

3. A human level of existence needs definition in more than structural terms, and the behavioral levels of adaptation observed in living primates are as legitimate a basis for reconstructing behavioral evolution as are deductions from comparative anatomy.

4. The term *protoculture* is introduced as a convenient term for conceptualizing a behavioral plateau that links the Hominidae with other primates. At this stage we have in rudimentary and unintegrated form many of the indispensable conditions for the development of culture in its unique form at the human level: learned behavior, biparental families, structured social groups, some form of communication, tool-using if not tool-making.

5. A further exploration of the distinction between a protocultural and cultural level of adaptation, giving full weight to the socio-psychological factors involved in a human existence, should make possible a more adequate and scientific definition of man.

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MORPHOGENESIS IN BACTERIA: SOME ASPECTS OF SPORE FORMATION

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INTRODUCTION

IF ONE contemplates the infinite resourcefulness nature has adopted to perpetuate its biological representatives, one can hardly avoid a profound reverence for the artfulness she has employed. Numberless are the mechanisms whose ultimate effect is to enable the species to adapt to its changing environment, yet none is more marvelous than that device which confers a relative indestructibility upon an organism, namely, sporogenesis in bacteria. To be sure, the remarkable resistance of the spore can be readily overcome with appropriately severe treatments, but if we envision the degree of adversity the spore is likely to encounter in nature, then in its context the spore does indeed display a virtual indestructibility.

This intriguing aspect has made spore cells the subject of an enormous amount of investigation since their significance was first disclosed by Ferdinand Cohn in 1876. As Cook (1932) pointed out, the celebrated controversy on spontaneous generation perhaps owed its long duration to the existence of spores. And, of course, it is hardly necessary to remark that almost the entire vast food-canning industry is geared to the thermal resistance of the bacterial spore.

It is my intention to examine here one aspect of bacterial spores, namely, the transformation of a vegetative cell to a spore cell—the actual metamorphosis.

CONDITIONS THAT INFLUENCE SPORULATION

Any approach to this subject inexorably becomes channeled into a consideration of the conditions under which bacteria will sporulate. Promptly one encounters an array of information so conflicting that the inexperienced investigator who is seeking a pattern common to all spore-formers might well become frustrated. Briefly, all this may be summarized by stating that (a) in a given environment different strains or species of bacteria express that capacity to widely varying degrees (Tarr, 1932; Velu, 1932), and (b) in different environments any given organism may sporulate to widely varying degrees.

If we think of sporulation as an all-or-none phenomenon in any one cell, it is obvious that such variations in sporogenicity will refer to the percentage of the vegetative cells present that sporulate, i.e., the "intensity of spore formation" (Buchner, 1890). Frequently the accuracy of such figures is jeopardized by the lysis of varying proportions of the vegetative cells before they sporulate.

In case (a) above, it is clear that intrinsic or "cellular" (Schmidt, 1950) factors condition the response of different organisms to a given environment. Furthermore, when in a uniform environment less than 100 per cent of the progeny of a given strain sporulate, it is clear that the population is heterogeneous with respect to this capacity. It is unnecessary to go into detail on these points; they have been described by numerous authors and summarized particularly well by Schmidt (1950).

Homogeneous environment

Intrinsic factors notwithstanding, it is certain that the discordance evident in collected results is not entirely without relation to inhomogeneous environmental conditions and the operation of factors not suspected to intrude on sporogenesis. A few years ago, when at the instigation of my colleague, Dr. O. B. Williams, we had occasion to become interested in this problem, it seemed that a useful starting-point would be to single out one organism and one set of conditions reproducibly permitting the great majority of cells to sporulate.

In attempting to arrive at our "ideal" system with aerobic bacilli, we discovered that misleading deductions were made about the capacity of a particular liquid medium to support spore formation when the culture was incubated in a stationary condition. Agar cultures and stationary liquid cultures behaved alike, regardless of how shallow the

layer of medium was. Sporulation percentage was much less and the maximum was greatly delayed, as compared to that obtained in liquid cultures shaken continuously during the growth period. Not only did shaking expedite the availability of oxygen to these aerobic bacteria, but the agitation ensured a homogeneous nutritional and physical environment for all cells at all times. The agitation technique for providing a homogeneous physiological environment is indispensable for adequately controlled sporulation experiments and is practically standard nowadays (Foster and Woodruff, 1944; Knaysi, 1945; Hayward, Marchetta, and Hutton, 1946; Foster and Heiligman, 1949b; Grelet, 1946a, b; Brewer et al., 1946). There can be little doubt that a great many of the sporulation experiments in the literature were limited by the "agitation-aeration" complex.

Nutritional influences

In experiments where the material was properly agitated, it became evident that the formation of spores was not an obligatory consequence of prolific vegetative development, but rather appeared to be a potential secondary process which a cell either displayed or did not. This secondary process is very sensitive to many physiological factors which have no appreciable quantitative effect on vegetative development, and sporulation can thereby be selectively inhibited (Wynne, 1952). Worth mentioning here is that sporogenesis is more fastidious than vegetative growth with respect to hydrogen ion concentration, temperature, concentration of certain inhibitors, oxygen tension, disinfectants, surface active agents, and certain salts.

In yet other ways it can be demonstrated that formation of a vegetative cell is not a sufficient condition for sporogenesis. For example, with respect to certain nutrients, potassium and manganese in particular, the requirements for sporulation exceed those for prolific vegetative development (Foster and Heiligman, 1949b; Charney, Fisher, and Hegarty, 1951; Curran and Evans, 1954). Addition of more of these minerals after full vegetative growth had taken place failed to provoke sporogenesis. Hence an excess of these cations is required during development of the vegetative cell in order to "condition" or predispose it to subsequent sporulation.

Not infrequently one finds mineral effects of the type just mentioned described as "stimulating" spore formation. This usage obviously is fallacious; the minimal requirements for sporulation are merely being satisfied.

Each of the above sporulation requirements was discovered in complex organic media which supported good vegetative growth but which were naturally deficient in the particular minerals. Grelet (1946a, 1950), employing a synthetic medium which supported good sporulation of certain strains of *Bacillus megaterium*, tested the effect of a deficiency of the individual components on that process. Sporulation failed to occur when potassium, magnesium, or manganese was limiting, and omission of calcium caused both a marked alteration in morphology and a reduction in spore formation.

There appears not to be any completely satisfactory evidence implicating for sporulation a requirement of organic substances or growth factors in amounts greater than adequate for maximal vegetative growth. Especially may this be said for those aerobic spore-formers which grow and sporulate well in synthetic media consisting of mineral salts and one or a few organic compounds.

A recent report that sporulation of *Bacillus subtilis* is related to the concentration of glutamic acid in the medium over and above that required for maximal growth (Krask, 1953) requires experimental clarification, especially since the nutritional requirements for growth and sporulation of this species, as well as *B. megaterium*, are satisfied by a glucose-salts medium (Knight and Proom, 1950; Grelet, 1946b). The situation is quite complex, for during growth the bulk of the glutamic acid undoubtedly is degraded, as contrasted to being utilized intact along with other intracellular amino acids in the metamorphosis (Hardwick and Foster, 1952; Foster and Perry, 1954). Thus, different functions are involved, and it may be that the latter process is more sensitive to the antimetabolite used. These comments also apply to the selective inhibition of sporulation in *B. cereus* caused by alanine (Foster and Heiligman, 1949a). On account of the difficulty of convincingly excluding indirect effects, one could very well justify the view that a study of sporogenesis under the minimal nutritional conditions which will support the process is perhaps the most direct route to an understanding of the essential features of the metamorphosis.

Special mention should be made of the extremely interesting finding that a nutritional factor extractable from pollen enhances the sporulation of

Bacillus larvae, the causative agent of the foulbrood disease of honeybees (Smith, Beck, and Anderson, 1949). This organism, like other insect pathogens which sporulate profusely in the host, e.g., *Bacillus popilliae* and *B. lentimorbus* (Dutky, 1947), is notorious for its sparseness of sporulation in the conventional complex laboratory media (Katznelson and Lockhead, 1944). However, the pollen experiments mentioned above have not been reproducible, and the published data did not take into account the extensive lysis of vegetative cells (J. V. Beck, pers. commun.); consequently this whole problem requires careful study.

Antisporulation factors

The vulnerability of sporogenesis to nutritional influences not demonstrably affecting vegetative development was unexpectedly encountered in other studies designed to ascertain why some complex organic media allowed good sporulation and others did not. In these cases minerals were not limiting. The work of Roberts and Baldwin (1942) opened the way to our understanding of how extremely complicated the matter of sporogenesis in complex organic media may be. They found that peptone media pretreated with activated charcoal led to a marked increase in the percentage of *Bacillus subtilis* cells which sporulated, as compared to the same medium not so treated. They suggested two possibilities: that the charcoal removes either some essential food factors, whose depletion provokes spore formation, or alternatively, removes some factor directly inhibitory to the spore-forming process. They did not establish which of these explanations applied. In a subsequent extension of this approach it was found that the latter alternative satisfactorily accounts for the effect (Foster, Hardwick, and Guirard, 1950; Hardwick, Guirard, and Foster, 1951). The results of Roberts and Baldwin were confirmed and extended to each of a variety of aerobic sporeformers, including *B. larvae*. The same influence was found to apply to several species of *Clostridium*, so that the phenomenon is very likely common among spore-forming bacteria.

The term "antisporulation substances" was used to designate the materials which are present in normal complex organic media and which suppress sporulation, meanwhile having at that concentration no marked effect on growth. One important class of antisporulation substances was isolated from complex media and identified as fatty acids. Testing of known fatty acids revealed that those possessing the greatest antisporulation activity were the saturated acids containing 10 through 15 carbon atoms. However, these certainly are not the only antisporulation substances, for media extracted with fat solvents could further be improved in sporogenic capacity by treatment with activated charcoal. Based on the observation of Olsen and Scott (1946), the incorporation of soluble starch in the medium led to absorption of antisporulation factors for non-diastatic sporeformers, including *B. larvae* (Foster, Hardwick, and Guirard, 1950).

Further complicating the sporulation picture was the revelation that various species of the genus *Bacillus* generate significant amounts of lipoidal antisporulation activity during their growth in complex media. The evidence suggests that the majority of this lipid may have been present in the original medium in a bound form unextractable by ether.

Besides an involvement of lipids preexisting in the medium, a net synthesis of lipoidal antisporulation substances during active growth of the bacteria might also contribute to the problem. Thus, a medium initially not containing an effective level of antisporulation factors could attain that level through growth of the bacteria, to result in a suppressive action on the subsequent sporulation of the culture. Before finishing this consideration of antisporulation factors in complex media, it is pertinent to mention that during chemical fractionation of complex media in an effort to separate the antisporulation factors the presence of other substances that neutralize or counteract the lipoidal antisporulation factors could be demonstrated (Hardwick, Guirard, and Foster, 1951).

Elucidation of the existence of antisporulation factors had the unexpected result of revising our interpretation of experiments used for many years to support a widely prevalent concept relative to the conditions which induce a cell to sporulate. Dating from 1890 (Büchner) is the theory that vegetative cells undergo sporulation when the available nutrients become exhausted, a kind of imminent starvation (Knaysi, 1945). Consequently, when sporulation of various organisms was found (Henrici, 1924; Williams, 1931; Brunstetter and Magoon, 1932; Tarr, 1932) to occur best in dilute complex media and progressively less in more concentrated media, an explanation quite logically was found in the "depletion of nutrients" theory. A more likely explanation is the sensitivity of the sporulation process to the

antisporulation factors which come into play in the higher concentrations of complex organic ingredients. Thus, charcoal treatment of asporogenic higher concentrations of complex media renders them sporogenic. This interpretation is further substantiated by experiments in which increasing concentrations of untreated organic nutrients resulted not in a decreased but in an increased percentage of sporulation (Brunstetter and Magoon, 1932; Brewer et al., 1946). Presumably antisporulation factors were not effective in these experiments.

To recapitulate up to this stage, it is apparent that the degree of sporulation is a resultant not only of the integrated action of the species and strain of organisms, but also of type of nutrients, of multiple types of antisporulation factors, and of substances antagonistic to at least some of the antisporulation factors.

Chemically defined media

Fortunately, it is unnecessary to handicap an experimental approach to the intimate nature of sporogenesis by the indeterminate system of variables characteristic of complex organic media. The sporulation of many typical aerobic bacilli takes place readily in the simple kinds of synthetic media. First to appreciate the desirability and perhaps the indispensability of synthetic media for analysis and reproducibility in sporulation work was O. B. Williams (1929), and now, since the first successes by Cook (1931) and by Tarr (1932), several synthetic media are known to be useful, particularly for aerobic sporeformers (Roberts, 1934; Foster and Heiligman, 1949b; Grelet, 1946b; Williams and Harper, 1951). One synthetic medium supporting good growth and sporulation of *Bacillus anthracis* contains 32 ingredients (Brewer et al., 1946). Considering that the nutritional requirements of some score of species belonging to the genus *Bacillus*, all capable of growth in media of defined composition, are now known (Knight and Proom, 1950), extensive sporulation studies in this genus under rigorously controlled conditions should now be feasible.

The sporulation of anaerobic bacteria in synthetic media apparently is not as successful as with their aerobic counterparts, but there already is ample reason for optimism that significant developments will occur in this area in the near future (Williams and Blair, 1950).

Selective inhibition of sporulation

The combination of shaking and the use of simple defined media, in which good sporulation of aerobic bacilli was obtainable in a relatively short incubation period, provided the basis for our experimental approach to the nature of sporogenesis. Thus, it was found that metabolism essential for spore formation is selectively more vulnerable than that adequate for formation of vegetative cells. For example, sporulation in three species of *Bacillus* was inhibited by 2 to 4 mg. of DL-α-, L-α-, or β-alanine per ml. of medium, without affecting growth, but the inhibition was not obtained with four other species (Foster and Heiligman, 1949a). Thus, under the conditions employed, these amino acids may be regarded as antisporulation factors. Similar results for other amino acids have been found in the case of other species of the genus *Bacillus* by Amaha and Sakaguchi (1952).

The antisporulation activity of DL-α-alanine was completely reversed by the presence of 0.5–1.0 mg. of yeast or liver extract, a level which had no significant influence on the amount of growth made. These experiments implied that the yeast and liver contained a substance (or substances) normally present in the vegetative bacterial cell and essential for sporogenesis. Either its synthesis or its utilization in sporogenesis must have been blocked by the toxic alanine, and the yeast or liver furnished it (or something convertible to it) and thereby restored sporogenesis. These experiments further extended the accumulating data which suggest that sporogenesis may be studied independently of vegetative growth.

ENDOTROPHIC SPORULATION

Kinetic studies on vegetative growth and spore formation in media of good sporogenicity reveal that the latter process takes place to a negligible degree so long as the growth of the bacteria is logarithmic or nearly so. It probably is not inaccurate to state that under homogeneous physiological conditions spore formation commences only after vegetative proliferation has virtually ceased, and, furthermore, that the great majority of those cells present sporulate within a relatively few hours. This is true in complex organic media (Henrici, 1924; Knaysi, 1946) and in synthetic media (Grelet, 1946a, 1950; Hardwick and Foster, 1952), and is particularly well illustrated by the time-lapse photomicrographs of Bayne-Jones and Petrilli (1933) and of Knaysi (1946).

These observations separating growth and sporogenesis in time, combined with the numerous works already cited, led us to suspect that sporogenesis could be physically divorced from the growth process, thereby making possible a direct study of sporogenesis uncomplicated by growth. It would be particularly useful to study the effect of imposed treatments, and to know that the treatments are affecting sporogenesis proper, in contrast to affecting the growing vegetative cell with respect to its subsequent capacity to sporulate. An analogy might be found in the technique of utilizing non-proliferating "resting cells" to study respiration uncomplicated by the multitude of reactions coincident with growth. As a matter of fact, the identical resting-cell technique was employed: vegetative cells from shaken cultures of various species of the genus *Bacillus* were removed from the synthetic growth media at a time well before the point of maximal growth and the onset of sporulation. The cells were washed in distilled water by centrifugation. Finally the cells were suspended in distilled water (or phosphate buffer) and placed on a shaking machine at 30°C., and the suspension examined microscopically at 2-hour intervals. Apart from a noticeable decrease in the size of the cells, no significant morphological changes could be detected during the first 6 to 10 hours, depending on the species, with an ordinary light microscope. Then the majority of the cells present sporulated within about a 2-hour period. Oxygen consumption during the period of metamorphosis showed no striking alteration associated with the transformation. These relations are depicted in Fig. 1. Under standardized conditions the sporulation time-pattern in distilled water is surprisingly reproducible. Spores produced in distilled water possessed the same heat resistance as spores produced in synthetic growth medium. As a convenient means of alluding

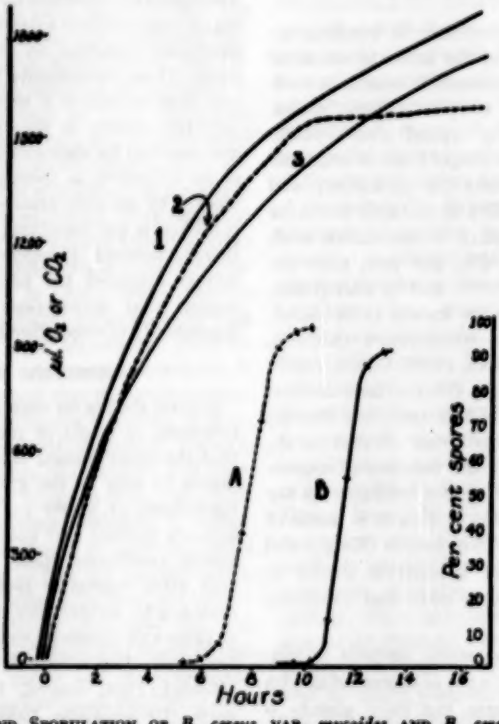


FIG. 1. RESPIRATION AND SPORULATION OF *B. cereus* VAR. *mycoides* AND *B. cereus* VAR. *lacticola* DURING CONTINUOUS SHAKING IN DISTILLED WATER

Curve 1, O_2 uptake by the *mycoides* strain; curve 2, O_2 uptake by the *lacticola* strain; curve 3, CO_2 produced by *mycoides*. Curve A, sporulation of *lacticola*; curve B, sporulation of *mycoides*. (From W. A. Hardwick and J. W. Foster, 1952.)

to spores produced in the absence of exogenous nutrition, and where growth is precluded, in distinction to those formed in growth media, the term "endotrophic sporulation" was proposed. Although this technique had not, so far as we can determine, been previously employed in a systematic study of sporogenesis, several investigators have in the past observed that vegetative cells of aerobic bacilli would sporulate after being placed in distilled water (Büchner, 1890; Schreiber, 1896; Knaysi, 1945, 1951).

Endotrophic sporulation is theoretically liable to the interpretation that a major proportion of the vegetative cells placed in distilled water die and undergo autolysis, with the survivors proliferating and sporulating at the expense of the lysate nutrients. In essence, this would represent sporulation in a dilute growth medium. Undoubtedly lysis does occur under some conditions, as Powell and Hunter (1953) have shown with certain strains of *Bacillus* grown on complex organic media and suspended in distilled water. There a massive lysis occurred. (J. J. Perry, in my laboratory, has also found that serious lysis occurred when he employed the precise conditions described by Powell and Hunter.) However, under the conditions in our laboratory there was no evidence of lysis of such proportions during sporulation in distilled water, despite assiduous attempts to reveal it (Perry and Foster, 1954). Moreover, it could be shown that within the limits of accuracy of the spread-plate counting method, heat-resistant spores were formed from the majority of vegetative cells initially present, without any serious change in cell numbers during the entire endotrophic process. Usually there was recovered on the plates about twice as many spores as vegetative cells in the original suspension. This presumably was due to the breaking up by the shaking of the 2-celled and 4-celled groupings which are characteristic of the vegetative cells of the common aerobic sporeformers. Had substantial lysis occurred, there would have been a substantial reduction in viable cell numbers at the end of the experiment.

Inhibition of endotrophic sporulation and its reversal

A curious effect of glucose (0.1–0.4 per cent in 0.1 per cent phosphate buffer at neutrality) led to a more detailed examination of the course of events in endotrophic sporulation. The results are illustrated in Fig. 2. Normal sporulation (no glucose added) was about 90 per cent. When the glucose was added after the 1st, 2nd, 3rd, 4th, or 5th hour, subsequent sporulation was completely suppressed. Glucose added to the cells shaken for 6, 7, or 8 hours was progressively less effective in suppressing sporulation, and when added at the ninth hour or after, was totally ineffective in suppressing sporulation. It was noted that spores were absent between the sixth and tenth hours. Apparently, during shaking in water, changes predisposing the cell toward spore formation take place before visible sporulation (using the light microscope) occurs. This is reflected in the change from reversibility of sporulation by glucose (up to the 6th hour) to irreversibility (6th to 10th hours). That aerobic bacilli removed from a growth

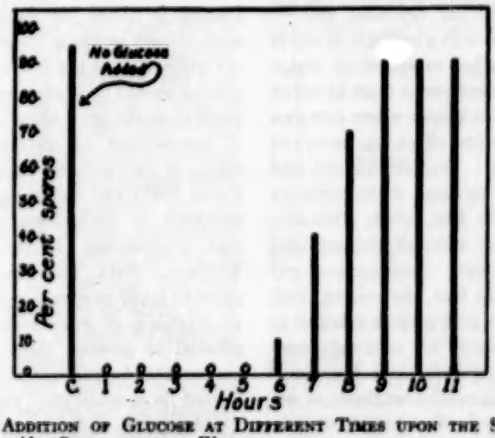


FIG. 2. EFFECT OF AN ADDITION OF GLUCOSE AT DIFFERENT TIMES UPON THE SUBSEQUENT SPOROGENESIS OF *B. cereus* VAR. *mycoides* SUSPENSIONS IN WATER

medium just prior to sporogenesis do become committed to sporulation even when fresh nutrients are furnished, had been previously observed by other investigators (Bayne-Jones and Petrilli, 1933; Grelet, 1946a).

When a small amount of ammonium hydroxide nitrogen was added to a glucose-treated cell suspension, the inhibition of sporulation by the glucose was appreciably reversed, e.g., about 50 per cent sporulation was now obtained. On the supposition that the presence of glucose permitted a synthesis of new vegetative protein at the expense of intracellular nitrogenous compounds of low molecular weight otherwise required for sporogenesis, experiments were undertaken to determine more directly if intracellular free amino acids are required for sporogenesis. A number of amino acid analogues, notably norleucine, cyclopentaneglycine, and *p*-fluorophenylalanine (inhibitors of methionine, isoleucine, and phenylalanine, respectively) were found to inhibit endotrophic sporulation. The inhibition was reversed competitively and specifically by the corresponding metabolite amino acids. Similar results were obtained with an inhibitor of adenine utilization, 2,6-diaminopurine. These experiments provided the first good evidence that intracellular compounds of low molecular weight must be available in order for sporogenesis to occur. They imply that to some extent the spore material is synthesized de novo from preexisting small molecules.

Intracellular precursors of spores: spore synthesis

Further evidence implicating the free amino acid pool in sporogenesis is provided by the fact that cells of *Bacillus cereus* var. *mycoides*, starved for nitrogen by passage through a medium in which nitrogen was limiting, failed to sporulate under endotrophic conditions. [However, it must be noted that at least one instance is known where nitrogen starvation had the opposite effect—it provoked sporulation (Grelet, 1950).] This experiment and the results are analogous to those of the Virtanen school (Virtanen and De Ley, 1948; Virtanen, 1949; Roine, 1947) where induced enzyme synthesis failed in low-protein (nitrogen-starved) cells of *Escherichia coli*. In fact, the results of all our studies on the amino acid pool in relation to sporogenesis in aerobic bacilli are strikingly reminiscent of those of Spiegelman and Halvorson (1953) in relation to induced biosynthesis of enzymes and its dependence on the intracellular pool of amino acids.

For example, when sporogenesis is inhibited by metabolite analogues, the inhibited cells, like those in which induced enzyme synthesis is inhibited, are not depleted in their content of free amino acids, whereas uninhibited cells are (Foster and Perry, 1954). Similar results were obtained with respect to the inhibition of utilization of intracellular purines and pyrimidines when 2,6-diaminopurine inhibited endotrophic sporulation. Additional observations from these experiments were: inhibition of sporogenesis by any single amino acid analogue or purine analogue prevented utilization of all of the different intracellular amino acids, purines and pyrimidines which normally would have decreased markedly during sporogenesis. Thus, inhibition of utilization of any one metabolite by its specific analogue results in inhibition of utilization of all the intracellular metabolites so far analyzed. This all-or-nothing utilization suggests the occurrence of de novo synthesis of protein and nucleic acids during endotrophic sporogenesis.

Actually, for several years there has been good evidence, mainly through the work of Lamanna, that certain biosynthetic activities are uniquely associated with sporogenesis. This conclusion is made on the strength of the discovery that bacterial spores contain antigens not detectable in the vegetative cells (Lamanna, 1940, 1942; Doak and Lamanna, 1948). The antigenic difference between a spore and its progenitor vegetative cell apparently resides both in polysaccharides and in proteins.

We have already concluded, from the results of the experiments in which glucose was present during endotrophic sporogenesis, that any factor tending to utilize the intracellular pool of amino acids should preclude sporogenesis. Theoretically, the utilization of the free amino acid pool by one process should preclude any other process also dependent on the pool. Already known were instances of competition for precursors during the synthesis of two induced enzymes (Spiegelman and Dunn, 1947), and the preclusion of induced enzyme synthesis by *Escherichia coli* cells infected with and synthesizing bacteriophage (Monod and Wollman, 1947). This mutual exclusion also appears to apply to sporogenesis and induced enzyme biosynthesis. A suspension of bacilli which was allowed to proceed through the first stages of endotrophic sporulation until the cells were "committed to sporulation," but had not yet formed visible spores, failed to respond to the addition of maltose as an inducer of the maltase system,

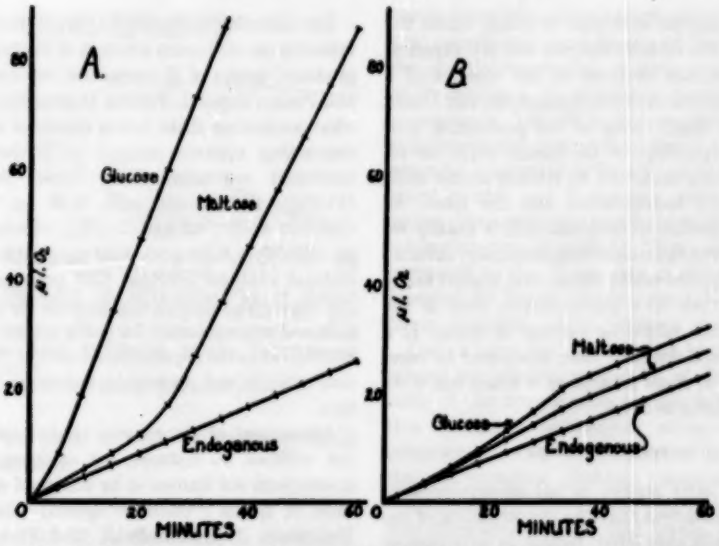


FIG. 3. THE OXIDATION OF GLUCOSE AND MALTOSE BY *B. cereus* VAR. *mycoides*
A, by washed cells. B, by another aliquot of the cells which had been shaken in water for 7 hours, i.e., until the "irreversibility of sporogenesis" set in. (From W. A. Hardwick and J. W. Foster, 1952.)

whereas cells "uncommitted" to sporulation were inducible (Fig. 3) (Hardwick and Foster, 1952). The most attractive interpretation here is that sporogenesis precluded maltase synthesis by making prior utilization of the intracellular amino acid pool on which the latter depends. The converse of this experiment is not critical, for a prior induction of maltase synthesis presumably results in the formation of glucose as an intermediate, and gives results indistinguishable from the earlier described glucose experiments.

One additional and perhaps more direct test of the idea of a synthesis de novo of spore material under endotrophic conditions was conducted with S^{35}-methionine. Using as the criterion of protein synthesis the conversion of methionine-S^{35} into an insoluble form, the rate of incorporation reached a discrete peak between the sixth and eighth hours (Fig. 4). This interval may well coincide with the period of prespore synthesis already assumed to occur, as judging from the failure of glucose to suppress sporulation after the fifth hour. The most active S^{35} fixation was estimated to occur about 2.5 hours before sporulation attained its maximal rate in this particular experiment.

It may also be noted that addition of known inhibitors of endotrophic sporulation, for example, norleucine, 2,6-diaminopurine and hydrogen ions (pH 5.0) each markedly reduced the fixation of S^{35}-methionine. Glucose, on the other hand, appreciably increased the fixation of labeled methionine, a finding in agreement with the interpretation that this carbohydrate promotes a protein synthesis competitive with spore synthesis.

The content of free amino acids in the vegetative cells of *B. cereus* var. *mycoides* is not sufficient to support de novo spore synthesis to a very large extent, and creates the necessity for regenerating at a sufficient rate the amounts needed for sporulation. Again, the situation here seemed analogous

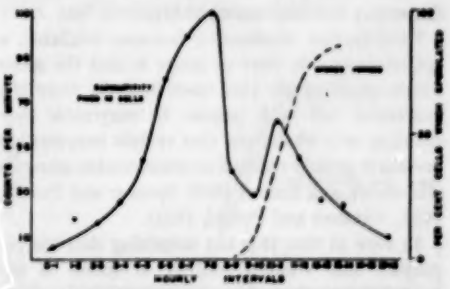


FIG. 4. THE RATE OF FIXATION IN INSOLUBLE FORM (PROTEIN SYNTHESIS) OF S^{35}-METHIONINE DURING SEPARATE HOUR INTERVALS DURING ENDOTROPHIC SPORULATION OF *Bacillus cereus* VAR. *mycoides*
(From J. W. Foster and J. J. Perry, 1954.)

to induced enzyme synthesis in yeast, where the synthesis of one induced enzyme was accompanied by a concomitant decrease in the content of a second preinduced enzyme (Spiegelman and Dunn, 1947). Accordingly, some of the preexisting proteins or polypeptides of the bacilli might be regarded as being sacrificed to furnish amino acids for subsequent incorporation into the spore. As most of the protein of bacterial cells is usually regarded as being functional enzymatically, actually or potentially, one might expect that a great many enzymes present at a characteristic level in the vegetative cells would be present in spores at a greatly reduced level, or that, compared to vegetative protein, spore protein as a whole has a reduced enzymatic activity.

ENZYMES IN SPORES

Although early studies in our laboratory indicated that numerous enzymes characteristic of the vegetative cells were either lacking or were present in greatly reduced amounts in the corresponding spores (Hardwick and Foster, 1953), a sufficient number of enzymes have since been found in spores of other species of bacteria (Spencer and Powell, 1952; Stewart and Halvorson, 1953; Lawrence and Halvorson, 1953; Levinson and Sevag, 1954) to permit the conclusion that spores do indeed contain some enzymes.

One may also pose the question, in cases where the same enzyme activity can be extracted from vegetative cells and from spores, as to whether the respective activities are indeed caused by identical enzymes. A case in point is the heat-labile catalase of vegetative cells of *Bacillus cereus* (*terminalis*) and the heat-labile catalase and the heat-stable catalase extracted from the spores of that organism (Lawrence and Halvorson, 1954).

Until further information becomes available, a not unreasonable view to adopt is that the spore differs qualitatively and quantitatively from the vegetative cell with respect to enzymatic constitution as a whole, and that certain enzymes are present in greatly reduced or undetectable amounts (Hardwick and Foster, 1953; Spencer and Powell, 1952; Virtanen and Pulkki, 1933).

In view of this, it is not surprising that the coenzyme and vitamin content of spores is appreciably less than that of vegetative cells. Such is the case of cytochrome, hematin, and flavinadeninedinucleotide (Keilin and Hartree, 1947; Spencer and Powell, 1952), and of each of the other five B vitamins tested thus far.

The following shows the content of the respective vitamins per milligram nitrogen of endotrophically produced spores of *B. cereus* var. *mycoides* (Perry and Foster, unpub.). Figures in parenthesis denote what percentage these values represent of the corresponding vitamin content of washed freshly harvested vegetative cells: biotin, 6.49 mμg. (23.0%); pantothenic acid, 0.20 μg. (45.5%); nicotinic acid, 1.62 μg. (15.3%); riboflavin, 0.22 μg. (17.3%); folic acid, 1.30 mμg. (12.6%). Additional analyses revealed that practically all of the respective vitamins contained in the vegetative cells and not accounted for in the spores was found in solution in the supernatant liquid of the suspension after it had undergone endotrophic sporulation.

Assessment of the enzyme content of spores is not without its hazards, for enzymes from the sporangium are known to be adsorbed on the surfaces of spores ("unclean" spores) (Stewart and Halvorson, 1953; Hardwick and Foster, 1953), and what might seem to be the enzymatic activity of the spores may in reality be due to contaminating protein. Furthermore, spores germinate with great rapidity (Hills, 1950; Powell, 1950), and in some circumstances there may be a question as to whether one is measuring the enzyme content of ungerminated or of germinated spores, the latter already resembling vegetative cells in their physiology. Also to be appreciated is the possibility that whatever the content of an enzyme in ungerminated spores, it may exist in a bound or otherwise inactive or "protected" form, a circumstance consonant with the relative heat-resistance of the spore and with the possibility that as such it may not display activity in spore extracts.

At any rate, the assembled data are best interpreted at this time as indicative of protein and nucleic acid turnover. At least a portion of the protein (and undoubtedly other components) of the vegetative cell appears to be sacrificed during sporogenesis, in consequence of which spore material becomes newly synthesized. Whether this results in no detectable content of a particular enzyme in spores, or in merely a reduced content, or in an increased level, is irrelevant to the hypothesis of intracellular turnover during sporogenesis.

Tracer experiments using S^{35}-labeled cells also suggest a turnover of preexisting protein in the vegetative cell undergoing endotrophic sporogenesis. Fig. 5 shows that there definitely was a release of radioactive sulfur and that the peak

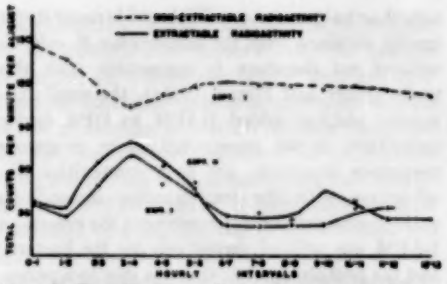


FIG. 5. THE RATE OF SOLUBILIZATION OF RADIOACTIVITY IN S^{35}-LABELED *Bacillus cereus* VAR. *mycoides* CELLS (PROTEIN DEGRADATION) DURING SEPARATE HOUR INTERVALS DURING ENDOTROPHIC SPORULATION
(From J. W. Foster and J. J. Perry, 1954.)

preceded the peak of active prespore synthesis already described.

DIPICOLINIC ACID

Early in 1953 W. A. Hardwick, in my laboratory, discovered that a hot 25 per cent ethanol extract of a suspension of germinated spores upon cooling deposited a crystalline residue. During the course of identification of this material by J. J. Perry (also in my laboratory), Powell (1953) described the isolation of dipicolinic acid (pyridine-2,6-dicarboxylic acid) (DPA) from germinated spores of *Bacillus megaterium*. Our compound proved to be identical with DPA, and thereby confirmed the discovery of the British bacteriologist. The problem of biosynthesis and the function of DPA in bacterial sporogenesis has been our interest for the past several months (Perry and Foster, 1955) and undoubtedly will occupy us for some time to come. We have developed a spectrophotometric micromethod of quantitative analysis for DPA after the extraction and elimination of interfering materials. The bulk of the DPA in ungerminated spores apparently exists in bound form, for an extraction in boiling $3N$ H_2SO_4 or $3N$ NaOH was essential to release the maximal amount. Extraction of large amounts of vegetative cells failed to reveal the presence of any DPA with this sensitive method. Consequently, DPA must be synthesized de novo during sporogenesis, even though traces of this compound might ultimately be detected in vegetative cells by means of an ultramicromethod. The synthesis of DPA was followed during the time course of endotrophic sporulation. As Fig. 6 reveals, the rate of synthesis of DPA was, within the limits of assay, practically identical with the rate of sporulation, the two curves being virtually superimposable. DPA formation presumably must be one of the final acts in the sporulation process, as there was no evidence of its existence in significant amounts prior to sporogenesis. The DPA content of sporangia-free endotrophically produced spores of *B. cereus* var. *mycoides* was found to be 4.8 per cent of the dry spore weight, and it was not much different in spores obtained from a synthetic medium and from a complex organic medium. These values may be compared to the 12 per cent of dry spore weight reported by Powell and Strange (1953) for a different organism under different conditions.

Only a very limited start has been made with respect to possible precursors of DPA. On the basis of the comparative biochemistry of formation of certain heterocyclic nitrogen-containing compounds through ring closure of the skeleton of the corresponding aliphatic compounds, for example, proline from glutamic acid and pipecolic acid from lysine (Vogel and Davis, 1952; Lowy, 1953), a potential precursor might be 2,6-diaminopimelic acid (DAPM) (Fig. 7). This compound, also known as α,ϵ-diaminopimelic acid, occurs in species of the genus *Bacillus* (Work and Dewey, 1953; Powell and Strange, 1954), and we have detected small amounts in acid hydrolysates of *B. cereus* var. *mycoides*.

Totally labeled C^{14}-DAPM isolated from the culture filtrate of the auxotrophic mutant of *Escherichia coli*, strain 26-26 (Davis, 1952) was added to a suspension of the aerobic bacillus undergoing endotrophic sporulation. The DPA isolated from the spores was radioactive, but no more than 4 per cent was derived from the labeled DAPM (Perry and Foster, 1955). This experiment neither establishes nor eliminates DAPM as a precursor of DPA, and testing of additional possible precursors

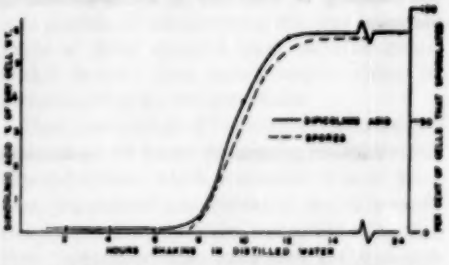


FIG. 6. THE CONTENT OF DIPICOLINIC ACID IN A SUSPENSION OF *Bacillus cereus* VAR. *mycoides* UNDERGOING ENDOTROPHIC SPORULATION
(From J. J. Perry and J. W. Foster, 1955.)

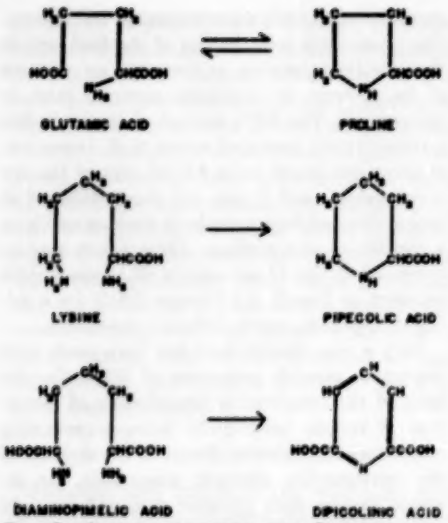
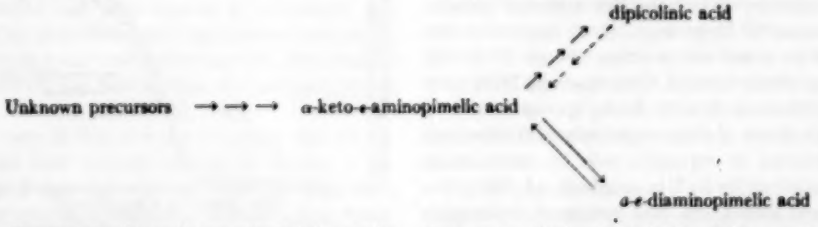


FIG. 7. ANALOGY BETWEEN THE DIAMINOPIMELIC ACID, DIPICOLINIC ACID SYSTEM AND THE GLUTAMIC ACID, PROLINE AND LYSINE, PIPECOLIC ACID SYSTEMS

continues. Discretely labeled DAPM would be particularly valuable for testing as a precursor.

The above experiments should not be construed to mean that α,ϵ-diaminopimelic acid is per se a direct intermediate in the biosynthesis of dipicolinic acid. In all probability the DAPM is converted to the true precursor, which, if one judges from the corresponding conversion of lysine to pipecolic acid, may well be the analogous monoketo compound, namely, α-keto-ϵ-aminopimelic acid. Just as α-keto-ϵ-aminocaproic acid, the monodeaminated product of lysine (which exists in the cyclized form as Δ'-dehydropipecolic acid) is converted to both lysine and pipecolic acid in *Neurospora* (Schweet, Holden, and Lowy, 1954), so can the following be visualized in the spore-forming bacteria:



Thus, diaminopimelic acid was selected for study because of its obviously close relation to the presumed actual precursor of DPA and because it was readily obtained from the auxotrophic *E. coli*. As pointed out elsewhere in connection with this study (Perry and Foster, 1955a), the small conversion yield of added DAPM to DPA during sporulation is not unexpected, since exogenous precursors frequently are at a competitive disadvantage with the corresponding intracellular active compound. In this experiment the exogenous DAPM was utilized very slowly by the bacteria, and the probability that this was due to a permeability barrier is strengthened by the finding that one exists for this amino acid in *Escherichia coli* (Davis, 1955). It may also be noted that Schweet et al. (1954) found a low conversion (4.6 per cent) of labeled dehydropipecolic acid to lysine and pipecolic acid in living *Neurospora* cells, a result they ascribed to slow penetration of the exogenous dehydropipecolic acid into the cell.

Of course, it is entirely possible that comparative biochemistry may mislead us here, and that the carbon skeleton of the labeled diaminopimelic acid is degraded, the pieces being resynthesized to dipicolinic acid. However, if degradation products did enter the metabolic pool, one might expect considerable radioactivity to appear in other spore components, whereas, as a matter of fact, none of the twelve amino acids isolated from the spores contained much radioactivity in comparison to the dipicolinate. Nevertheless, the data at this time are only suggestive, not conclusive.

We hope that the program currently under way in our laboratory, as well as in other laboratories, will substitute information for the present speculations on the biogenesis of this unique compound, dipicolinic acid, whose presence in spores is the first record of its natural occurrence.

Equally speculative is the matter of function of DPA, especially considering that it represents up to 12 per cent of the dry weight of the spore. One possibility which suggested itself was a relation to the distinctively high calcium content of spores found by Curran and his colleagues in

1943. One could conceive that DPA in spores exists as the calcium salt. However, calcium analyses performed on endotrophic spores have led us to conclude that these particular spores are not enriched in calcium in relation to their progenitor vegetative cells (Perry and Foster, 1955b). Furthermore, the calcium content was far less than equivalent to the DPA content, a ratio eliminating the possibility of a calcium salt of DPA. Unless one takes the view that such a large amount of DPA exists in the spore as the free acid, one is obliged to conclude that the carboxyl groups are masked. One could visualize the DPA existing as an anhydride, perhaps polymerized. On the other hand, one could visualize the abundance of carboxyl groups as being masked through combination with other organic components in the spore, perhaps even proteins. If this were so, one could surmise that through such a combination (including linkages with the pyridine nitrogen) the vital proteins of the spore are protected from inactivation during extremes of heat and other destructive influences. Or if a combination with nucleic acid is possible, a suppression of the synthesis of new heat-sensitive protein might be envisioned (Gale, 1953). It is premature and unprofitable to speculate on this matter any further, except to point out that in line with the foregoing, the function of diaminopimelic acid apparently is restricted to the ungerminated spore, that is, the resistant cell. Evidently its use to the cell is ended as soon as the spore becomes heat-labile through germination, for during that process the DPA is excreted intact and in large amounts (Powell and Strange, 1953), and thereby its utilization as a nutrient in the sense of carbon and energy is made unlikely.

EVOLUTIONARY SIGNIFICANCE OF SPORULATION

For the last topic of discussion it is appropriate to consider briefly the matter of the circumstances which induce a vegetative cell to sporulate. On account of the survival value with which a sporulating organism is seemingly invested, a strong teleological flavor has permeated many interpretations of this metamorphosis. The bulk of the evidence, provided by numerous investigators over the last half-century, indicates the existence of a close relation between spore formation and the exhaustion of nutrients essential for continued vegetative growth. This is an experimentally verifiable relation, and one quite germane to an understanding of sporogenesis. But it is anthropomorphic to interpret this relation, as several investigators have done explicitly, as one in which the cell sporulates in anticipation of survival during a period of inadequate nutrition or other viscissitudes. The best insight into this observed relation that I have found is that of Oginsky and Umbreit (1954): "The process of sporulation does, indeed, result in a mechanism for defense: not, however, a bulwark hastily erected when the enemy is sighted, but a fortress that happens to be there when the enemy arrives."

It is unnecessary to evaluate in detail here the matter of survival advantage that the property of spore formation confers upon a bacterium. Manifestly, mutational acquisition of the ability to produce a spore was the prime event in the evolution of spore-forming bacteria. An accidental property of the spore, namely, its resistance, presumably has been the chief reason for genetic selection of this type of organism in nature.

Nevertheless, there may be a danger in ascribing the evolutionary selection of the spore to its resistance *per se*. Sporulating forms represent only a small minority of all the bacterial species and, evolutionarily speaking, non-spore-formers have persisted as intimately mixed populations with spore-formers. Whatever the resistance mechanisms of the non-spore-formers, that resistance does not require spores. As a matter of fact, the distinctive feature of most bacterial spores is an ability to withstand environmental conditions so extreme that the operation of these extremes as selective influences in nature is at least debatable. Clearly, the selection is not against sporeless bacteria simultaneously exposed to the same environments.

It is also worth keeping in mind that resistance is not a necessary property of spores. Clonal spores exhibit greatly different resistances to any particular imposed stress such as heat tolerance, and it is possible, if not probable, that the large majority of clonal spores of most bacterial species freshly isolated from nature would exhibit resistances which are not remarkable.

Thus, our concept of "resistance" in connection with spores is based principally on laboratory-imposed stresses which a minority of most given spore populations can withstand, and it is easily conceivable that some feature of spores other than their "resistance" may have been the true basis for their genetic selection in nature. Indeed, an entirely novel basis of evolutionary selection of spore-

forming bacteria has recently been advanced (Bisset, 1950), and although that particular view is not without its obstacles (Lamanna, 1952), it is perhaps significant that it does not employ "resistance" *per se* as the rationale for genetic selection.

It is not feasible to speculate further on this point except to mention an alternative interpretation that is widely held among bacteriologists, and which, incidentally, is consistent with the hypothesis of spore formation outlined below. This view holds that spore formation is associated with a deficiency of available nutrients. Since the spore exhibits a relatively negligible metabolism, it has equally negligible nutritional requirements and consequently can survive periods of famine which would be fatal to the same organism if it did not sporulate. Thus it may be that under these conditions, the survival of sporogenic species is attributable to their ability to produce spores, whereas under the same conditions the survival of non-sporulating forms clearly must be attributable to some other properties. Accordingly, spore formation is only one of the characteristics, among different bacteria, which prove vital during periods of famine.

INDUCTION OF SPORULATION

In bacteria with an appropriate genetic constitution, the formation of a spore is a stage in the life cycle of that organism, just as various kinds of spores and cysts are in other biological systems. Even as in these other biological systems, so in bacteria too sporogenesis may either be expressed or not. Its expression depends on a fortuity of physiological and nutritional circumstances which, as we have seen earlier, mark the sporulation process as one of striking fastidiousness, since what suffices for vegetative growth may not be adequate for sporogenesis.

The spore is synthesized in consequence of a set of biochemical reactions, these dominating the metabolism of the cell when the internal physiological balance so decrees, and superseding the reactions leading to the synthesis of vegetative cytoplasm. In essence, spore synthesis and vegetative cytoplasm synthesis are mutually exclusive in any one cell—they represent alternative biochemical pathways in an actively metabolizing, genetically competent cell.

Now what of the more intimate aspects, from a mechanistic viewpoint? So long as hiatuses exist in our knowledge of the metamorphosis, any hypothesis necessarily undergoes revision to accommodate new facts, or may be abolished altogether. From the limited data of our experiments we have formulated a tentative view of the phenotypic pattern of sporogenesis, not with conviction that it is as indestructible as the spore, but in the hope that other investigators will take up experimental arms and assail it. Perhaps in the ruins there will remain something salvageable for our ultimate understanding of sporogenesis.

The primary event in the metamorphosis probably is an uncoupling of endergonic reactions from the exergonic reactions displayed wherever an exogenous substrate is utilized as an energy source. Essentially this means cutting off the supply of energy to the vegetative cell. The obvious case is when the energy-yielding substrate is all consumed. But it can also occur when utilizable substrate is still present. Considering the large number of enzymatic reactions involved in the generation of biologically useful energy and its coupling to endergonic reactions, one would assume that the sequence is liable at one point or another to interruption by numerous environmental conditions, such as temperature, hydrogen ion concentration, naturally occurring organic inhibitors, synthetic antimetabolites, salts, nutritional deficiencies, etc. If the energy supply is cut off, substances of high molecular weight become converted to low molecular weight substances, for it requires energy to conserve the preexisting complex components of the cell, that is, to maintain the equilibrium of the cellular system such that substances of high molecular weight preponderate. As Lees (1953) put it, "The maintenance of order in the labile pattern of the cell constituents necessarily involves the expenditure of energy (Schrödinger, 1944)."

It will be recognized that the intracellular degradations which commence upon exhaustion of the energy supply are not unique to sporeforming bacteria, but apply equally well to non-spore-formers, where usually they are induced also by energy depletion. In the non-spore-formers, one ordinarily describes the event as the initial stages of autolysis, and, indeed, our present views hold that the initial stages of sporogenesis are indistinguishable from autolysis. Now, whereas the autolytic processes may lead to death of the non-spore-former, the special feature of spore-formers is a mechanism whereby the intracellular products of autolysis are

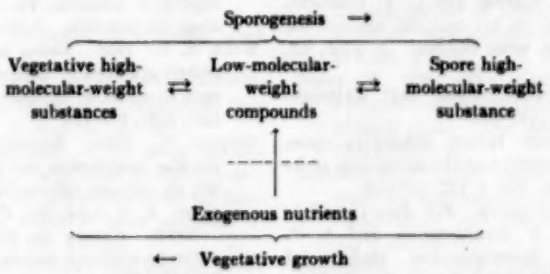
utilized to construct what eventually proves to be the survival mechanism—the spore. The organism survives because it forms spores; it does not form spores in order to survive.

Just what activates the synthesis of the new constituents, including at least some polysaccharides, proteins (Doak and Lamanna, 1948; Stewart and Halvorson, 1953), peptides (Powell and Strange, 1954) and dipicolinic acid, is pure speculation at the moment, but it would not be surprising if it were some kind of autoinduction process analogous to induced enzyme synthesis or perhaps even phage synthesis. Intracellular availability of one or more appropriate inducers would be a prerequisite. These would be derived from the preexisting vegetative cytoplasmic components, and would be made available fast enough to satisfy the new synthetic activity. The above events apply to situations where the only source of low-molecular-weight precursors of the spores is intracellular, namely, under endotrophic conditions and in simple synthetic growth media. Where exogenous substances of low molecular weight are present, as in complex organic growth media, undoubtedly exogenous spore precursors could also be utilized to an unknown extent, as indicated by the experiments dealing with the incorporation of radioactive methionine.

What proportion of the spore matter is synthesized de novo from vegetative components under endotrophic conditions, is a question impossible to answer at present. Certainly there is no evidence against some of the vegetative components of high molecular weight being incorporated intact into the spore. As a matter of fact, favoring this idea is the evidence that the spore contains, along with new antigens, antigens in common with the vegetative cell (Lamanna, 1940, 1942), and, of course, one may assume that nuclear material is also incorporated intact.

As mentioned earlier, the specific stimulus responsible for the induction might be the presence of adequate amounts of intracellular specific substrates at a time when competitive syntheses are absent. But the assumption of any synthetic process implies a need for energy. The energy would necessarily come from the breakdown of intracellular components, for this would be endergonic. Thus, a considerable portion of the vegetative cell would have to be oxidized to furnish energy for conversion of the remaining portion into spore matter. A material balance showed (Perry and Foster, 1954) that spores weigh less than 60 per cent of their progenitor vegetative cells, and additional evidence (Perry and Foster, unpub.; Lamanna, 1952) indicates the figure may be nearer 30 per cent. Although the dry weight of the spore has been thought by some to be considerably greater than that of the progenitor vegetative cell of the same organism (Powell and Hunter, 1953), our weight data are in line with extensive data showing that the volume of the spore is only about one-tenth that of its sporangium (Knaysi, 1946; Petrilli and Bayne-Jones, 1933).

The general relations we have been developing may be summarized in the following schematic form, the crucial factor determining the direction of this over-all biological equilibrium being a supply of exogenous energy, or the lack of it.



Thermodynamically, the net change from left to right is exergonic, with a net reduction in cell material. From right to left the change is endergonic, with a net material gain in cell substance.

In closing, it may still be true to say that a precise formulation cannot be given for the mechanism of metamorphosis of a vegetative cell into a spore cell, but by the same token there is reason for being sanguine about its further elucidation. Perhaps we may finally relinquish as an epitome of the ideas on this subject, the oft-quoted famous observation of Cook (1932) that "bacteria . . . form spores because they form spores."

ACKNOWLEDGMENTS

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will occasionally appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to H. B. Glass, Associate Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

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GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

SOVEREIGN REASON, *and Other Studies in the Philosophy of Science.*

By Ernest Nagel. The Free Press, Glencoe, Ill. $5.00. 315 pp. 1954.

This volume presents within a single cover 16 studies formerly published in reviews, philosophical journals, or symposium volumes. It thus makes available to general readers material formerly most easily accessible to professional philosophers. The essays include critiques of the ideas of Peirce, Dewey, Whitehead, Russell, Eddington, Reichenbach, Brand Blanshard, and other modern philosophers who concern themselves with the goal of reason and the nature of inquiry.

Nagel is one of that frighteningly small minority of contemporary writers on the philosophy of science who are sufficiently well versed in each of these disciplines to be able to speak with authority to experts in both; he also can express himself with sufficient clarity to be intelligible to both. There is, however, more to say of Nagel's contributions than that they are sound and lucid. They are almost overpowering in the tightness of their logic, and in their style they are brilliantly eloquent. While some of them are not easy to follow (notably the reviews of Peirce's books), by far the greater number illuminate to considerable depth not only the ideas of the subjects of the criticisms but also those of Nagel as critic.

One of the characteristics of philosophers seems to be a tendency, in which Nagel shares, to categorize classifiable modes of thought as *isms*. If the sovereignty of reason requires bolstering in the modern world, it seems to this puzzled reviewer that the sovereignty of criticism as a supreme intellectual feat also calls for some justification. Is there not something paradoxical in the custom of ascribing, in those very realms of intellectual endeavor which set the highest premium on innovation and imagination, a place of first importance to critical studies which are nourished as much from the ideas of other minds as from those of the critic himself? Such a practice would surely seem to devaluate the creative power of the mind if it did not so often transpire, at least in the field of literature, that the mind which excels in original thought is also the most acutely critical in the accustomed literary sense. Indeed, the literary critic might argue hotly that a distinction between the critical and the creative modes of thought is artificial; otherwise why that position of honor he has held for so many generations in the world of letters, at least among academicians?

But how does this concern Nagel and the philosophy

of science? It has to do with the fact that criticism in art and letters has no counterpart in science, unless the philosopher himself is the critic of science, and this is precisely the role that Nagel claims for him. He speaks of philosophy in general terms as an "endless task of criticism," and more specifically later declares that "in a readily identifiable sense, all of philosophy has been, and continues to be, a critique of science." In fact, one needs only to read Plato and Aristotle to confirm that from its beginnings criticism has been both the method and the style of philosophy.

The scientist, therefore, who inquires into the significance of his operations, both experimental and theoretical, can best do so under the guidance of a philosopher who, like Nagel, concentrates his attention precisely on the nature of inquiry itself. Through his critique of the work of the many influential figures concerning whom he is so instructive, both as to their positive contributions and their failings, Nagel develops also his own well-considered opinions. One of his strongest interests is to point out the fallacies of those who set up false dichotomies, and he himself is vastly occupied with endeavors to bridge the gaps between science and philosophy, between science and the world of usual experience, between science and ethics. He bends to his efforts a talent of consummate logical skill and he is a master of English prose style. All working scientists will therefore be wise to take his reflections into full account, and if they will permit themselves the privilege of reading this collection of essays they will enjoy the greatest of pleasures, namely, that of contributing to the preservation of the sovereignty of reason.

JANE OPPENHEIMER

THE BIOLOGY OF THE SPIRIT.

By Edmund W. Sinnott. The Viking Press, New York. $3.50. x + 180 pp. 1955.

In an earlier book, E. W. Sinnott advanced the thesis that there are two roads to truth, one through reason and one through faith or spiritual insight. To most of us, acceptance of this hypothesis—and obviously it can be considered only that—means that we also accept the existence of two kinds of truths, one kind measurable by the usual devices employed by the reasoning man and acceptable to all who care to verify them, and the other intangible and immeasurable but nonetheless real to the perceiver while at the same time nonexistent so far as others of opposite mind are concerned. The difficulties involved in the acceptance of intangible truths have plagued philosophers through recorded history, but Sinnott has apparently resolved these to his own satisfaction. The present volume is a detailed blueprint of his philosophical and religious beliefs.

Professor Sinnott is not a conventional believer; he rejects the traditional dogma associated with the Christian concept of a personal God at the same time that he cannot bring himself to the point of accepting either himself or his place in the scheme of things—physically and spiritually—as products of natural selection operating on the animate world. The latter process he dismisses as inadequate, if only because he believes that it cannot account for what he terms the "regulatory quality of living stuff," and because he further conceives of man's ability to judge abstract values, such as beauty and truth, to be without selective advantage. He here develops his ideas beyond the point reached in his earlier books, and in doing so concludes that man's mind as well as his body has a biological basis. Few scientists will dispute this conclusion, but to pinpoint this biological basis more specifically, Sinnott proposes that protoplasm is endowed with goal-seeking properties, with its highest present expression being the human spirit. A goal set implies a goal-setter, and in addition, "a Principle of Organization which, through life, brings order out of chaos, spirit out of matter, and personality out of impersonal stuff. This principle we may identify as an attribute of God."

The author bolsters his arguments with a description of the developmental patterns of various species, with the beautifully integrated harmony that exists at all stages of growth. This may impress those outside of the biological sciences, but the age-old question of faith versus reason will never be answered by the substitution of a scientific for an ecclesiastical dogma. The best answer still seems to be that which is inscribed in rock at the shrine of Lourdes: "If you believe in God there is no need for an explanation; if you do not believe in God there is nothing one can explain."

C. P. SWANSON

BERGSONIAN PHILOSOPHY AND THOMISM.

By Jacques Maritain; translated by Mabelle L. Andison; in collaboration with J. Gordon Andison. Philosophical Library, New York. $6.00. 383 pp. 1955.

This varied but thematically integrated series of essays, written over a span of some forty years, represents a classic in the history of ideas; and this for several reasons.

The major portion of the work is comprised of a translation of *La Philosophie Bergsonienne*, first published in 1913, as Maritain's first book as well as his critique of the philosophy of his former professor at the Collège de France, of whom Maritain could only have been called an ardent student. The ardent student, however, became a master in his own right, and is today no doubt the greatest living Thomist.

The critique analyzes Bergson's thought topically into the salient features of the Bergsonian system and examines these rigorously according to the Thomist

metaphysics. This is, then, primarily a metaphysical critique, and as such it follows each twist and turn of Bergsonism, both as manifested in Bergson's works and as evident in the *intent* or spirit of the structure of Bergson's thought itself.

Thus, though *Bergsonian Philosophy and Thomism* represents a personal declaration of Maritain's thought vis-a-vis his former master, and is one of the important works in an important philosophical controversy of the early 1900's, its greater and lasting significance is as an example of the exposition of a traditional philosophy as against a particular philosophical innovation. For it must be realized that the empiricism which Bergson confronted, and which presented itself as a *conclusion* of science and philosophical thought, essentially resembled the *problem* which presented itself to "Greek" thought. Bergson, in this light, may be presented as a Heraclitean with a vitalist cast.

Maritain, then, as a good historian and a vitalist of a different sort—St. Thomas says that the vegetative and animal souls are educed from the potency of matter—can be sympathetic with the attempted tour de force of a philosophy in which reality consists of vital duration and knowledge of the will-directed, "supra-intellectual intuition." Such an appreciation is presented in the Second Part of this book, which particularly considers the intentions of Bergsonian metaphysics and the views on morality and religion expressed in Bergson's *Les Deux Sources de la Morale et de la Religion*, which did not appear until 1932, twenty-five years after *Creative Evolution*.

Nevertheless, the exposition of the fallacies of Bergsonian philosophy so rigorously conducted in this book reveals the fundamental naiveté of a philosophy which, however noble its inner intentions, is constructed on false conceptions of reality, and inadequate distinctions. On the contrary, the critique reveals the resourcefulness, the strength, and the ultimate common sense of a realist and "open-ended" philosophy. The account of the Aristotelian-Thomistic synthesis is in the main excellent and fairly comprehensive. Particular attention should be drawn to the explication of the Thomist doctrines on time, which may, for the contemporary scientific mind, more readily provide an insight into the main stream of Thomist thought than the analyses of the Thomist positions on being and causality.

The rhetoric of this book, when it appears, appeals to the ethos of an intellectual sanity on which a man may pride himself, regardless of his philosophical position; but for Maritain, this intellectual sanity is not an exclusive possession of an enlightened few. He avows on the contrary its ultimate commonness as the implicit philosophical position of common sense.

The final portion of this book deals with several modern criticisms of Aristotle, and of St. Thomas as an interpreter of Aristotle. Was Aristotle an Aristotelian? Was Aristotle a Platonist? Was St. Thomas an Aristotelian? These questions may appear to be the result of the excesses of historicism, but they are dealt with by Maritain primarily as exercises in scholarship.

The translators have, in the main, done a laudable job with a writer whose style is both exacting and variable; and although the book suffers from the lack of a topical index, the publishers have done a service in making this classic available in English.

JAMES HILL

TRUTH AND FREEDOM. *Duquesne Studies, Philosophical Series, 5.*

By Louis de Raeymaeker and other professors of The University of Louvain. Duquesne University, Pittsburgh; Editions E. Nauwelaerts, Louvain. $3.00 (cloth); $2.25 (paper). viii + 133 pp. 1954.

This is a collection of essays written by seven professors of the University of Louvain in token of their respect to Columbia University on the occasion of its bicentennial. The theme is the ideal of full freedom for mankind in scholarly inquiry and expression and the right to the free use of knowledge so gained. It seemed very heavy going and full of cliches, and I can recommend it only to those who are metaphysically inclined. A fair example: "Literary critique is free and true when it takes as its model the sublime word of Bérulle: 'Because nature belongs to God, we will leave it without ruining it.' " (from the essay on Freedom and Truth in Literary Critique, by C. Moeller).

DAVID B. TYLER

SPONSORED RESEARCH POLICY OF COLLEGES AND UNIVERSITIES. *A Report of The Committee on Institutional Research Policy.*

American Council on Education, Washington. $1.50. viii + 95 pp. 1954.

This report of the Committee on Institutional Research Policy contains too much gobbledygook to be taken seriously. The Committee starts off very piously by setting forth what all know to be the function of institutes of higher learning, namely, the extension, conservation, and diffusion of knowledge, but ends up half apologetic and half conciliatory in regard to what to do about classified research. One cannot compromise on a basic principle without eventually discarding it into a garbage can. Any activity that imposes restrictions on the function of a university is not to be tolerated. Classified research, with its restrictions of secrecy, is completely foreign to the spirit of a university no matter what attempts are made to segregate this activity to "off campus locations." This research properly belongs in governmental or industrial installations that are committed to such activities, and trying to

justify its inclusion in a university's program by high sounding verbiage is double-talk. A university can discharge its responsibility to the community and to the national security only by behaving as a university.

DAVID B. TYLER

THE PHYSICS AND CHEMISTRY OF LIFE.

By the Editors of the Scientific American. Simon & Schuster, New York. $1.00 (paper). xii + 270 pp.; ill. 1955.

FIRST BOOK OF ANIMALS.

By the Editors of the Scientific American. Simon & Schuster, New York. $1.00 (paper). xii + 240 pp.; ill. 1955.

The Scientific American needs no introduction to either scientist or informed layman, and it would be no exaggeration to state that it has come to occupy a unique place in our monthly literature in keeping the reading public abreast of developments in a variety of scientific disciplines. In a very real sense, it stands for scientific reporting at its best, with clarity, accuracy, and pertinence as constant watchwords. The high standards which have characterized the revitalized journal since shortly after the termination of World War II have not been lowered with the passage of time, and each new issue is as eagerly awaited as the preceding one has been enjoyed.

The editors of the journal have now seen fit to issue a number of the articles in an inexpensive paper-bound book form. Those listed above are two of the five already in the book stalls. To be sure, the grouped articles do not form a continuous and complete story of any single scientific field, but they do give a reasonable picture of current thought and emphasis in two general areas of biology. In the first listed volume, 18 articles are grouped into six major topics: the origin of life; the molecules of life; the molecule of heredity; enzymes and energy; cell and organism; and muscle, nerve, and brain. Here such eminent scientists as Wald, Kamen, Pauling, Crick, Beadle, Mazia, Szent-Györgyi, and others tell of their work and its bearing on life itself. The volume, according to the editors, is addressed primarily to non-scientists, and in particular to priests, philosophers, and poets. It should give pause to those who, relying on mystical insight as the principal road to truth, conclude that life can never be resolved and comprehended in physical and chemical terms. The evidence suggests otherwise.

The First Book of Animals does not present as unified a picture as the volume just considered, but it is no less interesting or absorbing. Its major divisions include the balance of nature, mating, the social insects, the origin of species, physiological engineering, flight and navigation, insects in the laboratory, and the proper study of mankind. The names of Petrunkevitch, Tinbergen, Lack, Krogh, Griffen, Williams, and others emphasize the caliber and standing of the contributors.

Both volumes are splendid additions to anyone's library, and it is to be hoped that they reach the wide audience they so greatly deserve. They do suffer in one respect, however, when compared to the originals which have appeared in the *Scientific American*. They lack the imaginatively done illustrations which make the journal so unique a piece of monthly literature, even though one can appreciate the reason for their omission, since to include them would have appreciably raised the cost of publication.

C. P. SWANSON

BIOLOGY: HISTORY AND BIOGRAPHY

BRITISH SCIENTIFIC LITERATURE IN THE SEVENTEENTH CENTURY.

Edited by Norman Davy. Barnes & Noble, New York. $2.50. 244 pp. 1954.

A 40-page historical Introduction, of which only 6 pages are devoted to Zoology, Physiology and Anatomy, and Botany, is followed by sections containing selections from the great British 17th century scientists, arranged under the heads: General Science; Botany; Zoology; Geology; Astronomy; Meteorology; Sound (or Acoustics); Light (or Optics); Heat; Chemistry and General Properties of Matter; Magnetism; Technology; and Translations. There are appended Notes; a consideration of The Contemporary Attitude towards Science; Modern Works relating to British Seventeenth-Century Science; a Chronological Table; and an Index. The aim, the laudable one of acquainting students of the history of science with the great achievements of the noble group of British scientists in the era of its initial greatness, is rendered difficult by the restriction to selections written originally in the English language, for much of the greatest scientific writing of the time was published in Latin. Although the biological selections represent John Ray, Nehemiah Grew, Martin Lister, and Francis Willoughby, the selections are not distinguished, in part because much of the best writing of Ray and Willoughby was in Latin. Moreover, the great anatomists and physiologists, William Harvey, John Mayow, Thomas Wharton and Thomas Willis, and the great physician Thomas Sydenham, are excluded by the same restriction. What is most unaccountable is that neither Robert Hooke's *Micrographia* (biological portion) nor any of Edward Tyson's anatomical work is represented, even though the language was no barrier here. Yet the small volume, particularly in the selections from the physical sciences, contains much matter of interest and profit, even though the great classics are absent even there.

BENTLEY GLASS

A CENTURY OF PROGRESS IN THE NATURAL SCIENCES, 1853–1953. *Published in Celebration of the Centennial of the California Academy of Sciences.*

California Academy of Sciences, San Francisco. $1.00 (paper). xii + 807 pp.; text ill. 1955.

The year 1953 marks the 100th anniversary of the California Academy of Sciences, and the present volume is issued as a commemoration for the occasion. The selection of a theme which would appropriately do justice to the Academy itself and to the fields of endeavor which it represents must have given the committee assigned to this task no end of difficulty; but as the editors point out, the middle and later parts of the 1800's saw the publication of two ideas which were to revolutionize biology in all of its ramifications. These were ideas revolving around the theory of evolution as propounded by Darwin and Wallace, and the theory of inheritance according to Mendel. It took, to be sure, some time and a good deal of evangelism to gain widespread acceptance of Darwinism and Mendelism, and it has only been within the last quarter-century that the systematists have made use of these ideas to further their attempts to construct a system of relationships and phylogeny which could be shown, experimentally, to rest on a natural basis. However, the foundation of biology, whether experimental or descriptive, rests upon a knowledge of relationships, and the editors have chosen to honor their Academy by collecting a series of papers which deal with the historical aspects of biosystematics, and which reflect the influence of modern ideas on the progress made in the several areas of taxonomy.

The result is a thoroughly commendable volume, and one which should be read by every biologist regardless of his field of immediate interest. The editors should be congratulated on their success in securing such a wealth of talent. In the plant fields the following authors are represented: Van Niel (bacteria and bluegreen algae), Papenfuss (algae), Bessey (fungi), Steere (bryophytes), Manton (ferns), Florin (gymnosperms), and Constance (angiosperms). Twenty-three sections, and nearly as many authors, have dealt with systematic problems in the insects, Karl Schmidt handles herpetology and animal geography, Sibley ornithology, Hamilton mammalogy, Weaver invertebrate paleontology and historical geology, Good plant geography, and Leopold conservation. It is a volume worthy of the Academy.

C. P. SWANSON

THE PRINCE OF BOTANISTS, CARL LINNAEUS.

By Norah Gourlie. H. F. & G. Witherby, London. 30s. xiv + 292 pp. + 9 pl.; text ill. 1953.

This is the second biography of Linnaeus to appear within the brief time of 2 or 3 years. Like Hagberg's earlier volume, the present one covers the entire span of the great botanist's life, and reveals many interesting details of his personal and scientific career. The two biographies, in fact, are quite similar to each other, and it would be difficult to state fairly that one is better than the other. Miss Gourlie's approach is sympathetic without being adulatory, where controversy exists she presents such evidence as is available in a judicious manner, and the illustrations, while few in number, are pertinent to the text. While clearly a competent and workmanlike piece of writing, the volume cannot, however, be considered a distinguished biography, since it adds little in the way of fact or interpretation that has not already been said.

C. P. SWANSON

BIRDS AND MEN. *American Birds in Science, Art, Literature, and Conservation, 1800–1900.*

By Robert Henry Welker. The Belknap Press of Harvard University Press, Cambridge. $5.75. x + 230 pp. + 21 pl. 1955.

Prior to 1800, there was little in the way of natural science studies which could rightly claim to have furthered our knowledge of America. The colonists were too busy hewing out a living from the wilderness, and circumstances in the cities were not yet favorable for the encouragement of either science or art. The next hundred years, however, produced a number of men of scientific stature, and it is of those who were interested in ornithology that R. H. Welker writes.

Two men were largely responsible for establishing ornithology on a reasonably firm footing in the United States: Alexander Wilson and James J. Audubon. They had in common a love of birds, but in all other respects they were poles apart. Wilson was a dour man, self-trained, persistent and meticulous; he has been called the father of American ornithology. Audubon was the genius, favored by birth, training, social graces and artistic ability; his gifted brush portrayed American birds as no other has done before or since. In some instances he made use, consciously or unconsciously, of a number of Wilson's drawings, improving them, to be sure, but rarely giving credit for his debt. Both men were forced to seek their own subscribers, Wilson for his *American Ornithology* and Audubon for his great folios; both were discouraged and disheartened by the reception given them by the centers of American culture, although Audubon gained a reputation in his time that Wilson never achieved. Welker gives a good account of these men as they did their studies and as they sought acceptance in a period of American history which, to say the least, was not congenial.

Others came later to foster ornithology through literature, poetry, and conservation. Thoreau was probably the most influential of these, but the author also discusses the roles of Emerson, Burroughs, Whitman, and

a number of lesser lights in terms of their contributions and relations to the social and scientific climate of the times. The later period of the 1800's produced none who could compare with Wilson and Audubon, and this volume, interesting though it is, might well have been terminated much sooner than it was so far as its value to ornithological history is concerned.

C. P. SWANSON

W. H. HUDSON.

By Ruth Tomalin. Philosophical Library, New York. $3.50. 144 pp. + 1 pl. 1954.

FROM PAMPAS TO HEDGEROWS AND DOWNS. *A Study of W. H. Hudson.*

By Richard E. Haymaker. Bookman Associates, New York. $5.00. 398 pp. + 1 pl. 1954.

These two books together provide an excellent portrait of William Henry Hudson and a critique of his work. The little volume by Ruth Tomalin is a nicely written, glowing account of the life and character of the lad who, born of American parents, grew up on the Argentine pampas and spent his mature years in England. The author has caught very well the wonderful splendor and sweep of the grasslands and the multitudinous calls and songs of the rich variety of birds which so illuminated and quickened the senses of W. H. Hudson that many years later, in the crowded imprisonment of London boarding-houses or in the limited variety of the English countryside, he could still see and hear the enchantment of Nature and voice his abiding love for the out-of-doors in the here and now as well as the "far away and long ago."

The second of these books about Hudson is very different in character. The events in the life of Hudson play a small role here, being restricted mostly to a short introductory chapter that is by no means so full an account as the one by Ruth Tomalin. The bulk of the book is devoted to literary criticism of Hudson from various points of view. The author demonstrates an enormous breadth of general knowledge as well as an intimate understanding of birds and animals, of the British scenes where Hudson rambled and about which he wrote, and of the writings of Hudson's friends and contemporaries, as well as the out-of-door's essayists of other times and places. The introductory chapter on The Out-of-Door Essay is in fact a uniquely interesting critique of a type of literature that has been neglected by most students of literature. Other aspects of Hudson's work which Haymaker has considered include the course of Hudson's development and achievement; his literary artistry and poetic gift; his treatment of landscape; his knowledge of biological principles and the permeation of his mind by the concept of organic evolution; his appreciation of animals, including the insects, and especially the birds; his portraits and sketches of people; his own internal attitudes toward life and his keen appreciation of the senses; the special quality of his novels and tales, from *The Purple Land* to *Green Mansions* and *Tales of the Pampas;* and finally, his religious pantheism and mysticism, words which would seem more adequately to characterize his personal views than the phrase "religious atheist," which is used by the author.

Few writers in the borderland between natural history, belles lettres, and fiction have received more loving and thorough attention from those who have written about them than Hudson has here received at the hands of Ruth Tomalin and Richard Haymaker. For those of us who have acquired a deep, nostalgic fondness for the writings of W. H. Hudson, these books will occupy a special place of honor on the bookshelf.

BENTLEY GLASS

THE STORY OF MEDICINE.

By Arthur L. Murphy. Bouregy & Curl, New York. $3.00. x + 243 pp.; ill. 1954.

A SHORT HISTORY OF MEDICINE.

By Edwin H. Ackerknecht. The Ronald Press Company, New York. $4.50. xviii + 258 pp. + 16 pl.; text ill. 1955.

THE STORY OF MEDICINE.

By Kenneth Walker. Oxford University Press, New York. $6.00. 343 pp. + 27 pl.; text ill. 1955.

These books on the story of medicine should appeal to different audiences. Murphy's book is, by its style and content, suited principally for the lay reader. Ackerknecht's is a concise account of the progress in the science and art of medicine from man's early primitive attempts through early Asiatic, Egyptian, and Greek civilization into the Dark Ages and Renaissance and up to the present time. It is written to meet the need of medical students or busy doctors who wish to become oriented in this fascinating topic without going through more detailed treatises. This one is a well-written and well-organized book. The book by Walker covers essentially the same ground, and is somewhat more detailed and better illustrated than Ackerknecht's. It is admirably suited for medical students or physicians interested in the story of medicine.

DAVID B. TYLER

A PARIS SURGEON'S STORY.

By Charles F. Bove, with Dana Lee Thomas; foreword by Leland Stowe. Little, Brown & Company, Boston and Toronto. $4.50. xii + 306 pp. 1956.

Charles F. Bove is an American surgeon who was attached to the American Hospital in Paris during the period between the two World Wars, and on into 1940 when the Germans overran France. He managed to get out of Europe through Lisbon, and is now practicing in

New York. Although Dr. Bove is an authority on the treatment of burns and on transfusion techniques, the account of his stay in Paris is really the biography of a city, and contributes little in the way of technical knowledge. It is, however, a highly entertaining and readable volume, for the Paris of that era reflected, even as it does now, the troubled pulse of an embittered world waiting for disaster. Dr. Bove, an observant and sensitive man who can record with verve, humor, and clarity what he sees and feels, was in a position to meet the high and the low in station. Personalities in art, politics, and finance, as well as in medicine, people his pages, and give to the book variety and range. It makes for a light book, but one well worth the reading.

C. P. SWANSON

THE YOUNG NATURALIST

SCIENCE AND ITS BACKGROUND. *Second Edition.* *By H. D. Anthony. St. Martin's Press, New York; MacMillan & Company, London.* $4.00. x + 337 pp.; ill. 1954.

Once upon a time, when the world was fair and people were young, your reviewer was young too, and she went to school, and when she was in the equivalent of what are called conventionally the sophomore and junior years of high school—they were called something different in the small New England school she attended—she learned history: first, ancient history; next, medieval and modern history; American history was reserved for the last year of all. She had excellent teachers, and especially the teacher of the second course, that in medieval and modern history, she would like to describe as one of the two best teachers she has ever had in what has turned out subsequently to be a rather long course of education. (Her name was Miss Brock, and I wish I could believe that in this fairy-tale world of coincidence in which we *really* live, she might chance to read this belated notice of her virtues.)

Why is it said that she was a good teacher? Because when she taught about events, she taught not only about facts but about ideas; when she taught about ideas, she taught about them not only with words but also with feeling, and we students somehow learned to develop a sense of history without being able to put into words just what this sense was or what it grew out of. In fact, when your reviewer's fairy godfather some years later for the first time spoke the word history to her in the same breath as the word science, though she had since her schooldays read all too little of technical history, the resources within the well of her mind were deep to draw on, for which she had the training of her earlier years to thank.

Now, a particular teacher has been singled out as some one exceptional, as indeed she was. But she could never have made quite the success she did with medieval and modern history had not her predecessor, who was also a remarkable woman, had unusual ability to bring the ancient world to life, as was indeed the case, since this rather small school in New England had the good sense to view knowledge of both the ancient world and of at least one of its languages for just exactly what it is worth to the modern world, namely, a very great deal.

If there were two people who were then able, in this retrospectively utopian period of intellectual youth, to make history alive, what were the gifts with which they had been endowed by *their* fairy godmothers and which enabled them to do so? While their primary talents were in themselves, they were not exactly impeded in their efforts by the fact that they had had books of quality to assist them. The facts concerning events were accurate in these books, which meant that the teachers could talk also about ideas. But the books also contained ideas, which meant that the teachers could encourage their students to think about ideas and to develop some of their own. I should like to quote the first paragraph of the preface of one of these books.

"In the selection of subject matter," it began, "as well as in style and diction, it has been the purpose of the author to make this book sufficiently simple to be put into the hands of first-year high-school pupils. A great deal of labor has been devoted to the mere task of clear and simple statement and arrangement. While simple enough for first-year high-school work, it nevertheless is planned to interest and stimulate all students of high-school age. In dealing with each civilization a sufficient framework of political organization and of historical events has been laid down; *but the bulk of the space has been devoted to the life of man in all its manifestations*—society, industry, commerce, religion, art, literature. These things are so presented as to make it clear how one age grows out of another, and how each civilization profits by that which has preceded it." (The italics, by the way, are those of the original author.)

Clearly, this is the material and the method of intellectual history; and regard for intellectual history, and thus for the dignity of ideas in all fields, was what we learned. One reason we could do so was because our teachers could depend on the fact that the book just quoted, on ancient history, and another on medieval and modern history, attained the aims specified in the paragraph just reproduced.

Why were these books able to succeed? Surely because they were written not by high school teachers, nor by doctors of education, but by great historians. The one from which the quotation above was extracted was written by James Henry Breasted, the other by James Harvey Robinson.

Now what were the gifts with which Breasted and Robinson had been endowed by *their* fairy godmothers that enabled them to write such successful books? These historians were great men not only because they were professors at Chicago and Columbia, and not only be-

cause they opened new worlds to the investigation of professional historians, but also because they put a high value on the potentialities of the youthful mind. Because they were perceptive of the unfathomable depths of its capacities, they could move it to its profundities. They could take the trouble to sound the young mind, and in so doing they could learn to speak to it with the same eloquence with which they addressed their peers. In fact, their books written for high-school students may be read as limpidly and with as much profit today (and indeed are, by your reviewer) as in the golden age of enchanted childhood.

Your reviewer is not yet exactly a thousand years old, but time seems to have cast a spell, and textbooks of the caliber of these seem harder to come by. Their style was unique and has rarely been equalled. Yet there is an echo in the phrases of H. D. Anthony's *Science and its Background* which seems reminiscent of something out of an almost forgotten past—something that might indeed have been completely forgotten were it not for the re-readability of the books under discussion. Anthony's tone, in diction if not completely in style, is in many ways similar to that used by Breasted and Robinson when they spoke to young minds in which they had confidence. Since this author Anthony had been granted by his fairy godmother the opportunity to take his turn as Headmaster of a British Grammar School, he has had the good fortune to come close enough to those who are young to learn to appreciate (as it is indeed always appreciated in fairy tales) just exactly how receptive young minds can be.

This is not to claim that Anthony is another Breasted or Robinson. That is a beneficence reserved only for the very, very few, touched by the most special diamond and uranium magic wand. The godmother who blessed Anthony used a more mundane instrument, and his book, though in some ways admirable, has in others some failings. He has presented more an alternation of science and history, in his arrangement, than a fusion of them. His examples, which must be highly selected in order to keep the volume to a sensible size, are not always judiciously chosen: Piltdown man, for instance, is hardly to be presented uncritically as a serious reality in a book bearing 1954 as its publication date. And the proportions between philosophical background and scientific application are not always felicitously maintained. Anthony limits his discussion of the Renaissance to considerations of its forward-facing aspects, with never a mention of its obligation to its classical past; neither the word nor the idea of humanism finds a place in his presentation. Yet he allows himself 5 pages in a small book to devote to full details concerning jet propulsion. All the same, Anthony has carried through two endeavors of high significance: he has presented science to his readers as an integral part of history; and he has chosen for his readers an important audience, an audience whose minds are young and formative.

Great things are now happening for the history of science. Golden apples and plums are beginning to fall its way from *its* fairy godfathers, the Foundations; and golden thrones are being set up in its name in the great Universities. Yet gold will no more exalt it than would riches in the fairy tale have benefited the bad prince in his ordeal had he lacked valor. And where now are the valiant and handsome young princes fit to vie for the plums and ready to ascend the thrones?

They are enrolled in the schools, ready and eager to learn, whether they expect themselves to contest for the thrones or remain content to stand by and hearten the competitors, fully cognizant of the fact that they, and their children as well as the champions, stand to gain from the intellectual achievements of the latter if only they be understood. We who are old enough to read and write fairy tales are mostly too old to interrelate history and science, who have learned them as separate. That is a task for the young to accomplish who now live in an era in which science and history have become part of each other more than ever before. But they can do so only if there will be more Anthonys who are willing to speak to them when yet they are young. The world is still fair and the young are still brave; but no crystal ball is required to warn us that an understanding of the place of science in history must now become available to the young if they or we are any of us to live happily ever after.

JANE OPPENHEIMER

EXPERIMENTING IN SCIENCE.

By Gerald S. Craig, Ruth Lippenberger Roche, and John Gabriel Navarra. Ginn & Company, Boston, New York, Chicago, Atlanta, Dallas, Columbus, San Francisco, Toronto, and London. $2.60. 288 pp.; ill. 1955.

This is the sixth in a graded series of elementary books prepared under the guidance of Professor Craig of Teachers College, Columbia University. Like those which preceded it, it is a class book with an accompanying teacher's manual. It deals with the natural world around us: energy, seasonal changes, plant growth, astronomy, conservation, etc., and suggests a variety of simple experiments of sixth-grade level which should be most useful in bringing the child into closer contact with his immediate surroundings.

ECOLOGY AND NATURAL HISTORY

THE UNDERWATER NATURALIST.

By Pierre de Latil; translated from the French by Edward Fitzgerald. Houghton Mifflin Company, Boston. $3.50. 275 pp. + 15 pl.; text ill. 1955.

Many, if not most, of the books produced by underwater explorers have conformed to the general pattern of episodes about hunting fish for game, sunken treasure, and the like, with a minimum of solid information. The author of this book has tried to redress the balance with an account of the habits of fishes observed by him on the French Riviera. Much of the book is a literary review, better written than most such works, of published information, and perhaps not as much as is implied consists of original observation. Nevertheless, the book is a decided improvement, and a promise of better things to come as the novelty of diving wears off and the more hard-working biologists devote themselves to this type of investigation. There are, in addition, some delightful passages about such famous fish stories as the alleged Roman custom of fattening morays with human slaves and the muddles of famous ichthyologists.

JOEL W. HEDGPETH

BIOLOGY OF DESERTS. *The Proceedings of a Symposium on the Biology of Hot and Cold Deserts Organized by The Institute of Biology, London.*

Edited by J. L. Cloudsley-Thompson. The Institute of Biology, London; Hafner Publishing Company, New York. $3.00. iv + 224 pp.; ill. 1954.

The 28 papers in this volume cover geographic, economic, ecologic, and climatic aspects. Deserts and sheepgrazing in Australia, the role of insects in the Sahara, plant and other problems in the arid regions in Israel, human adaptation to extreme heat and cold, and other subjects are considered in detail. The individual contributions range in length from 4 to 20 pages.

Although the term "desert" is not amenable to strict definition, the implied definition around which the discussions in this symposium flow is that a desert is an arid region. Arid regions occupy about one-fifth of the surface of the globe and have always excited the interest of scientists and travelers because of their dryness, their extremes of temperature, and the peculiar adaptations of plants, animals, and men living within their confines. Nonetheless, scientific books and comprehensive reviews dealing with deserts are not numerous. For this reason, the present volume is a welcome addition to the literature, the more so since each contribution is by a recognized authority. There are papers in this volume which will interest students of all branches of science. Such topics as water conservation in small desert rodents, reactions to great heat, physical aspects of dry deserts, availability of ground water, plant ecological problems, the productivity of deserts, and man-made deserts such as our own American Dust Bowls represent just a few of the subjects covered. This volume is recommended as an authoritative and fascinating account of some of the more challenging environments of the world.

V. G. DETHIER

THEODORE ROOSEVELT'S AMERICA. *Selections from the Writings of the Oyster Bay Naturalist.*

Edited by Farida A. Wiley; with introductory essays by John Burroughs, Gifford Pinchot, Henry Fairfield Osborn, and Ambrose Flack; and a foreword by Ethel Roosevelt Derby; illustrated by Ugo Mochi. The Devin-Adair Company, New York. $5.75. xxvi + 418 pp. + 1 pl.; text ill. 1955.

Teddy Roosevelt, to the average man, was President, Bull Moose candidate, the man with the big stick, or the soldier who led the charge up San Juan Hill. To a somewhat smaller audience, he is also a man who was intensely interested in the natural world around him. This was indicated not only in the constructive efforts he made in the field of conservation and forestry, but also through a rather large volume of writings that include a number of books and personal journals, and cover travels and hunting expeditions in the West, South America, and Africa. He had the energy and the drive to immerse himself in whatever he was doing, and this quality is strikingly evident in his writings. In the present volume, Miss Wiley has selected particular portions from his books, arranged more or less in chronological order, so as to reveal the many-sided man who, to so many, "seemed to incarnate the soul of America." In addition to an Introduction by the editor, brief notes on Roosevelt as a naturalist, and written by Flack, John Burroughs, Gifford Pinchot, and Henry Fairfield Osborn, are included. The black-and-white illustrations by Ugo Mochi are appropriate and complementary. The result is an admirable book for the naturalist's library.

C. P. SWANSON

THE NATURAL HISTORY OF NORTH AMERICAN AMPHIBIANS AND REPTILES. *The New Illustrated Naturalist.*

By James A. Oliver. D. Van Nostrand Company, Princeton, Toronto, London, and New York. $6.95. xii + 359 pp. + 12 pl.; text ill. 1955.

American herpetology is fortunate in having an extensive literature. It is especially rich in "handbooks," which may include both the amphibians and the reptiles or, with more detail, only a single order or suborder. The great majority of these compendia cover the forms from the United States and Canada, although two recent excellent works by Stebbins are restricted to the herpetofauna of the western half of North America.

With one significant exception, in all earlier works the material is arranged systematically, and in essentially the same tradition as in Hegner's phylum-by-phylum approach to general zoology. The book under review stands in the same relative position to herpetology as does to general zoology that textbook by Shull, LaRue, and Rutheven in which the "principles" approach was introduced in 1920. *The Natural History of*

North American Amphibians and Reptiles, therefore, requires more than a routine notice.

For a work of this type, Oliver has had a very favorable background of wide and first-hand experience. He has been making natural history observations on amphibians and reptiles over the years while residing in Missouri, Texas, Michigan, New Hampshire, and Florida. For the past several years he has been Curator of Reptiles at the New York Zoological Gardens.

The book cuts across phylogenetic lines and considers 7 major topics: distribution, locomotion, activity, ecology, feeding habits, reproduction, and growth. There are also introductory chapters on folklore, economic values, classification, and short descriptive summaries of the kinds of amphibians and reptiles. A final chapter discusses many aspects of these animals as pets.

Oliver has a pleasant, almost conversational, style of presenting information. Reference to the contributions of others is solely by name and without the usual distracting details of a full citation. The book reads almost as if Oliver were reporting on informal discussions he had had with knowing herpetologists. Sprinkled among these résumés are a number of original observations and occasional reinterpretations of the views of other students. There are over 70 effective new drawings by Lloyd Sanford and a dozen interesting photographs, most of which have not been published previously.

The other major work, referred to above, that does not follow a phylogenetic plan is Noble's *Biology of the Amphibia*, and it might be compared with the present work. There are so many important differences between them, however, that such a comparison loses much of its instructiveness. Noble's work is on a world-wide basis and treats, in detail, such topics as anatomy, embryology, histology, instinct, etc., that are not covered by Oliver. The treatment given by Noble indicates that his book was intended for a more sophisticated audience. Oliver's book is "for the naturalist and the beginning student of amphibians and reptiles. No knowledge of biology is necessary to understand its contents." Noble's volume had a consistent theme—that of elucidating the phylogenetic relationships among the Amphibia. There is no similar unifying thread in Oliver's book.

In evaluating a contribution of this sort, its relation to the literature of the field has to be considered. The book is certainly an excellent pioneering attempt. It will satisfactorily serve a broader audience than that for which it was written. It is clear that the pertinent herpetological literature has been carefully surveyed and its factual material has been fairly presented.

In addition to such a compilation, I would have hoped for more syntheses, correlations, and generalizations than Oliver was able to find. It is probably unfair to look for them in a pioneer work of these dimensions. Certainly the volume will find a continuing use as a source book and will in the future stimulate synthetic interpretation of natural history data in herpetology. There is a glossary, an index, and a short list of major references.

ARNOLD B. GROBMAN

ON THE BIOLOGY OF SOME JAVANESE LORANTHACEAE AND THE ROLE BIRDS PLAY IN THEIR LIFE-HISTORIE. *Beaufortia Ser. misc. Pub., No. 41, Vol. 4, Oct. 20, 1954. By W. M. Docters Van Leeuwen. Zoological Museum, Amsterdam.* $2.00 (paper). ii + Pp. 105–207 + 10 pl.; text ill. 1954.

The flowerpeckers (Dicaeidae) are known to disseminate the seeds of some of the Loranthaceae (mistletoes), but the descriptions of the exact methods employed have been at variance. It is reported in this publication that the birds eat the fruit, swallow the pulp and seed, and discard the skin, often by rubbing it off with the bill. The birds consume a number of seeds and remain sitting quietly while digestion takes place. The process is rapid, having been timed from 12 to 22 minutes. On defecating, the bird rubs the anus on the perch and the seed becomes stuck to the substrate, adhering firm by its sticky covering. The body of the bird sags, jerks, and moves sideways as the seeds are eliminated one by one. After the last seed has passed out the bird flies away. In a short time the seeds sprout and commence growing into the host tissue.

Certain species of these plant parasites are pollinated by birds, although a few are entomophilous, and in others, self-fertilization may occur. The evidence in regard to pollination is scanty, but there seems to be no doubt that at least the majority of the Loranthoideae are bird-pollinated flowers, while probably the Viscoideae are pollinated by insects.

Although the review pays most attention to the role of birds, a topic ably and critically analyzed by the writer, it does include much biological information on the development and habitus of the plants, mainly those of the East Indies. Over a dozen species are included in the more detailed discussions and descriptions.

HENRI C. SEIBERT

LOUISIANA BIRDS. *By George H. Lowery, Jr.; illustrated by Robert E. Tucker. Louisiana State University Press, Baton Rouge.* $5.00. xxix + 556 pp. + 40 pl.; text ill. 1955.

The revival of popular interest in natural history has fostered the production of a number of state bird books in recent years. These have been written for those who already are interested in birds and, as a consequence, are for the most part detailed, stodgy summaries of the local ornithology. Aside from being of little interest to the uninitiated, the studies are technically weak and of slight value to the ornithologist. *Louisiana Birds* is a

refreshing change. Rather than attempt to write a book which will appeal to the amateur and also be of value to the professional, an impractical but often attempted approach, the author has narrowed his sights to create a work which will "introduce the people of Louisiana to the absorbing subject of ornithology." This he has done with great success and considerable charm.

The first fifth of the book contains deftly written chapters on the physical virtues of Louisiana (many), the history of ornithology in the state (distinguished), and the role of the Louisiana State University Museum of Zoology (important), as well as sections on the techniques for identifying birds, the biology of birds, and the methods by which birds are protected, attracted, and managed. The material is presented in an elementary, but stimulating, manner. It should enhance the novice's enjoyment of the sport of bird-watching and, perhaps, encourage him to try some of the more rewarding aspects of ornithology.

The remainder of the text is concerned with the species found within Louisiana. Each order and family is prefaced by a few remarks on its characteristics, affinities, and general distribution. The accounts of the species are informal, but informative, combinations of local natural history and instructions on how to recognize the birds in the field. It is commendable that subspecies are not treated, except for a few distinct forms.

Illustrations abound. The color plates are mostly of the "field guide" type, designed to facilitate identification. They are adequate and occasionally good. There are nearly 100 full-page or half-page photographs of birds and of habitats within the state. All are excellent in technical quality and beauty. Several hundred text figures and marginal sketches are useful and very decorative.

Through its interesting text, superb format, and low price, *Louisiana Birds* cannot fail to attract new devotees to bird-watching. It may well set the pace for future state publications.

RAYMOND A. PAYNTER, JR.

EVOLUTION

THE ORIGIN OF VERTEBRATES.
By N. J. Berrill. Oxford, at the Clarendon Press. 25s. viii + 257 pp.; ill. 1955.

This volume immediately takes its place beside Willey's classic work on *Amphioxus and the Ancestry of the Vertebrates*. In the more than half a century since Willey's book was published, nothing remotely approaching it either in scope or authority has appeared until now. Interest in the topic has been perennial since the 1880's but a tabulation by decades of the publication dates in the rather full bibliography reveals a marked upswing in that interest beginning in the 1930's and continuing to the present.

Berrill writes from a lifetime of original investigations into the structure, development, and interrelationships of the tunicates and related forms. His book is both thoughtful and thought-provoking. It brings together under a single point of view an enormous mass of data, old and new. Good use is made, for example, of the recent work of Carlisle showing that extracts of the subneural gland of tunicates, long regarded on anatomical grounds as homologous to the vertebrate pituitary, will in fact induce ovarian enlargement and other typical effects of pituitary hormones when injected into amphibians and mice.

The thesis Berrill presents is in part new and in part the extension of ideas that have been slowly forming for many years. Where Willey, working on the presuppositions of the recapitulation theory, regarded the sessile tunicates as "obviously degenerate," and Gregory more recently looked on them as a blind alley off to one side of the main line of chordate evolution, Berrill regards the tunicates as the basic deuterostome stock from which amphioxus and the vertebrates have evolved. According to this view, the sessile filter-feeder with its ciliated gill apparatus is the primitive form. The tunicate tadpole is a larval adaptation which gave rise by neoteny to transitional forms more or less like amphioxus which could exploit the rich detritus descending rivers. From this it was but a step to the first vertebrates, which most paleontologists now agree arose in fresh water.

It is a plausible and well-documented thesis. Unfortunately, all the chief events involved antedate the earliest fossils. Berrill wisely concludes by saying, "And so a story that hangs together in a logical way and includes most of the relevant facts is perhaps all that we will ever possess. Proof may be forever unobtainable." A prudent reviewer can then agree with one of the more down-to-earth characters in Gilbert and Sullivan's *The Gondoliers:*

"Of *that* there is no manner of doubt—
No possible doubt whatever."

GAIRDNER B. MOMENT

THE UNLEASHING OF EVOLUTIONARY THOUGHT.
By Oscar Riddle. Vantage Press, New York. $4.50. xxii + 414 pp. 1954.

This is a provocative assemblage of arguments selected to support two related themes: (1) that "naturalism, not supernaturalism, sweepingly defines man's relation to the universe"; and (2) that there are "several areas of everyday life in which the contributions of all science to logical thought are manhandled and perverted by prevailing religion."

Here are over 400 pages packed full of quotations, interpretations, and opinions. Part I, What Evolutionary Thought Is, includes such chapters as From Earth-Cloud to Man, The Problem of Creation, Evolu-

tion and Ethics, Social Inheritance, and The Biological Inequality of Man. Part II, The Reins Held by Religion, contains chapters entitled School: The Sepulcher of the Intellect, Religion's Power to Divide, Question Marks among the Alleged Assets of Religion, and 2 chapters on The Constitution on the Church and State in Education. Then follows a shorter Part III, Opinion and Outlook, in which are presented some of the public and private reactions to Riddle's previous expressions along these lines.

Despite the obvious dangers of quotation out of context, the following is one of many which might be cited to show the breadth of argument:

> [In the United Nations] the men who bring and must resolve these [political and moral problems of all countries] share no common view of either the origin and nature of man or the sources of values and morality. This is an outcome of the tragic tardiness and deadly failure of education to press unmitigated evolutionary principles clean through religious diversity and across national boundaries. In the whole Western world, reason and duty have called for positive and aggressive action since, at least, the publication of Darwin's book in 1859. . . . The extensions and dimensions of biology's foes in the spheres of politics and government—all clearly born of religion—can be illustrated by four brief quotations (p. 196).

The quotations which follow at this point are from a Select Committee of the (British) House of Commons dated 1879, from William Jennings Bryan, from Governor "Ma" Ferguson of Texas in 1926, and from the President of the Congress Party in India in 1950 (favoring the protection of the cow even at the expense of the country's economy). The timeliness of many of the evidences cited throughout the book shows a similar irregularity.

In the face of the flood of arguments presented by this eminent retired research biologist, the teaching biologist, even of the same philosophical persuasion, might wonder (1) how he has escaped an inquisition from "organized religion" or a governmental investigation for teaching evolution constantly, both implicitly and explicitly; and (2) whether the striking claims for the salutary effects of evolutionary thought do not lend fuel to the fires of current aspects of anti-science.

Modern thought admittedly finds incompatibilities between religion and science, or, as Oscar Riddle equates them, between naturalism and supernaturalism. The many recent books by scientists, designed to present to the educated layman their individual metaphysics, reflect the scientists' recognition of their responsibilities in this dilemma. Frequently such scientists as R. S. Lillie, R. A. Millikan, E. W. Sinnott, J. B. Conant, and others, recognize a duality intrinsic in the temporary or permanent limits to the understanding of matter-energy. In Oscar Riddle the strict materialists have a sincere and impassioned champion who presents "an account of unmitigated evolution, [in which] the natural sources of morality and values—or at least their non-requirement of a separate and 'spiritual world'—must now be particularly emphasized because this point is so uniformly denied in religion, because it is so shamelessly surrendered by many scientists, and because the entire structure of evolution has neither consistency nor any deep significance without it" (p. 92).

E. L. WHITE

GENETICS AND CYTOLOGY

GENETICS IS EASY. *A Handbook of Information. Second Edition.*

By Philip Goldstein. Lantern Press, New York. $4.00. xvi + 238 pp.; ill. 1955.

Genetics, starting with the elementary principles and extending to fairly recent work on biochemical and microbial genetics, is indeed made easy by this book. The style is extremely simple, and there are many helpful diagrams and charts. The presentation is based on specific examples and in this way brings the reader closer to the actual experiments on which the science rests. It is to be regretted that the author has seen fit to use the two-strand model of crossing over for explanation. It is also unfortunate that he has occasionally departed from standard terminology. For example, "monoploid" is used in place of "haploid." The most serious shortcoming is that the book is too much concerned with the details of the subject and too little with the broader implications of genetics for biology and human society. The latter receives very inadequate treatment. This is especially regrettable if this is the only book on genetics that its readers will see. However, if it stimulates them to read more widely then it will have given a good basic background for that reading. In general, the author has succeeded quite well in simplifying a college course in genetics to the level of the general reader, even that of the high school student. The fact that a second edition is needed is an encouraging sign that the book has found an audience.

R. F. KIMBALL

SEX - DETERMINATION. *Third Edition. Methuen's Monographs on Biological Subjects.*

By F. A. E. Crew. Methuen & Company, London; John Wiley & Sons, New York. $1.50. viii + 68 pp. 1954.

At the time of its first publication, in 1933, this brief account of the nature and mechanisms of sex determination was outstanding. It received a bit of revision in 1946, and now is reissued as a "Third Edition, Revised," with only a few changed lines and added references. It is true that this aspect of genetics has not undergone any fundamental revision of concepts in

recent years; and hence this review remains a good statement of the general principles of sex determination. Nevertheless, much recent work on problems of sex determination has improved our understanding of particulars. There is no mention herein of the existence of sexual recombination in bacteria; of Pipkin's search for the postulated sex genes in the X-chromosome and the autosomes of *Drosophila melanogaster*, or of Sturtevant's maternally conditioned intersexes in *Drosophila* species hybrids; of Warmke's and Westergaard's more recent work on the determination of sex in *Melandrium*; of Gordon's analysis of the transformation of the sex-determining mechanism in the platyfish from XX-XY in wild strains to ZZ-ZW in the "domesticated" variety; of Mackensen's analysis of sex determination in the honeybee; or of numerous other studies of the past 10 years.

This is still a useful book for anyone who wants a clear and concise summary of the earlier developments in our knowledge of sex determination. For those who want a more comprehensive view, Crew's review should be supplemented by the accounts given by Max Hartmann in his *Allgemeine Biologie* (4th ed.) and by M. J. D. White in his *Animal Cytology and Evolution* (2nd ed.), and by a critical survey of the genetic literature since 1945.

BENTLEY GLASS

METHODS OF PLANT BREEDING. *Second Edition. McGraw-Hill Publications in the Agricultural Sciences.*

By Herbert Kendall Hayes, Forrest Rhinehart Immer, and David Clyde Smith. McGraw-Hill Book Company, New York, London, and Toronto. $8.50. xii + 551 pp.; ill. 1955.

There are comparatively few treatises of plant breeding despite its enormous importance and the abundance of material for discussion. *Methods of Plant Breeding*, by Hayes, Immer, and Smith, is among the best of the books in this field of applied science. The first edition, written by H. K. Hayes and the late F. R. Immer, was an eminently successful textbook and reference work. The second edition, by Hayes, Immer, and Smith, maintains the high standards set by the first edition.

The organization of the current edition follows, in general, that of the first, except for several new chapters, notably those on Heterosis, Special Techniques, Development of Methods of Corn Breeding, Chi-square Tests, and Heritability. The new material has increased the length of the book to a total slightly more than 100 pages over the first edition (432 pp. vs. 551 pp.).

The first edition was criticized on the basis that it was too provincial, since its subject matter was limited almost entirely to the cereal crops and a few forage crops. An attempt has been made to eliminate this objection by adding a chapter on Cotton and Sorghum Breeding, and sections on Sugar Beets, Sugar Cane, Sunflowers, Onions, and Cabbage. Chapters on one of the vegetable crops, tomatoes, for example, about which a great deal is known, and perhaps on an ornamental crop, might be suggested as worthwhile topics for a future edition.

The chapters on statistical methods and on field plot technique have been revised and brought up to date. These are probably the most useful ones in the book, at least for the beginning student. The discussion of Heritability could have been improved by reference to the important work of Lerner, and of Lerner and Dempster, in that field. There are about 1000 literature citations, an appendix of statistical tables, a good index, and a glossary.

THOMAS W. WHITAKER

INVESTIGATIONS ON GENETIC ASPECTS OF CARCINOMA OF THE STOMACH AND BREAST. *Univ. Cal. Publ. pub. Health, Vol. 2, No. 4.*

By Charles M. Woolf. University of California Press, Berkeley and Los Angeles. $1.00 (paper). iv + pp. 265–349; ill. 1955.

This is the record of a carefully conducted study dealing with human material. As in all such studies, where human beings are the source material, gaps in knowledge and inaccuracies in the data that are secured are inevitable. These have been eliminated in so far as it is possible, by checking all records, where available. A total of 222 gastric cancer patients and 216 breast cancer patients, together with their parents and sibs, formed the basis of the study. The observed number of deaths among the relatives from gastric and breast cancers was compared with an expected number of such deaths, based on proportional mortality rates in the general population. In addition, the breast cancer relatives were compared with a control population, for extimation of the frequency of breast cancer in the two groups. The results support the hypothesis that there is a heritable component in both of these malignancies.

Relatives of gastric cancer patients exhibited a higher frequency of gastric cancer than was found in the general population. The difference was significant, with a probability of less than 0.001 that the difference was caused by sampling variations. Other types of malignancies occurred in these families no more frequently than expected, indicating that the hereditary factor present is organ-specific in nature, and not one for cancer in general. The frequency of stomach cancer in the 540 spouses of these stomach cancer patients was no greater than that found in the rest of the population.

Relatives of women with breast cancer had a higher frequency of breast cancer than women in the general population. Here the P value was less than 0.01. The author states that although there is a genetic component in both these kinds of cancer, the quantitative effect is small, and leads to the conclusion that exoge-

nous factors are of greater etiological significance. No simple pattern of inheritance was noted, and the author proposes that multiple genes are responsible in man, as has been found to be true of mammary carcinoma in the mouse. When the combined effect of genetic and non-genetic factors surpasses a critical threshold, cancer develops. I would comment that, coupled with the assumption of a polygenic origin of cancer, the fact that man has a markedly heterogeneous genetic background lessens the force of the statement that the quantitative effect of the heritable component is small. The failure of parent and child, or sibs, to exhibit the cancer in question in appreciable proportions may be due to the lack of necessary genes to complete the picture rather than to the fact that exogenous factors play the more important role. Without the aid of a uniform genetic background such as exists in inbred strains of mice, the assignment of relative roles or weight to heredity and to non-genetic factors may be fraught with error.

MADGE T. MACKLIN

GENETICS AND THE INHERITANCE OF INTEGRATED NEUROLOGICAL AND PSYCHIATRIC PATTERNS. *Proceedings of The Association, December 11 and 12, 1953, New York, N. Y. Res. Publ. Ass. Res. nerv. ment. Dis., Vol. XXXIII.*

Edited by Davenport Hooker and Clarence C. Hare. The Williams & Wilkins Company, Baltimore. $10.00. xii + 425 pp.; ill. 1954.

This 1953 annual session of the Association for Research in Nervous and Mental Disease must have been, from these published accounts of the proceedings, one of the most interesting meetings held by that society. It is too bad that space does not permit a detailed review of all the contributions, for most of them are worthwhile. L. H. Snyder presents an excellent summary of the present status of the science of genetics in relation to behavior. He cautions those who assume that heredity has a dominant role in determining the variability of any trait. B. D. Davis, in discussing the Genetic and Environmental Control of Enzyme Formation, points out the similarity between the changes in adaptation to penicillinase production and those involved in learning. B. E. Ginsburg presents the evidence (from the study of susceptibility to sound-induced seizures) that hereditary factors may act directly upon the central nervous system through its metabolism. A. B. Sabin points out that resistance or susceptibility of the nervous system to certain viruses can be inherited but that genetic control is only one of several variables that can influence susceptibility. Anne Anastasi speculatively discusses the interaction between environmental and inherited components of behavior. Josef Warkany brings up the controversial question of the influence of dietary deficiencies in producing specific structural changes in the nervous system of the embryo, a point that Windle objects to. Leonard Carmichael attempts to correlate the phylogenetic development of the brain with changes of behavior patterns. However, it is difficult to reconcile his thesis with the similarity of learning data obtained with a variety of forms (insect, birds, mammals) with marked differences in brain structure. Davenport Hooker presents data illustrating the sensory interaction in the fetus and shows that the inability of a child to discriminate one of two stimuli may not be the result of inattention. Tryphena Humphrey establishes an anatomical pattern for the early fetal movements discussed by Hooker. W. R. Thompson presents data that support the premise that intelligence is inherited but that the environment can exert permanent effects on this basic inherited capacity, particularly a severely restricted environment in early life. Nash Herndon evaluates the evidence that genetic factors are of importance in various lipidoses. G. A. Jervis summarizes the genetics and biochemistry of phenylketonuria, a condition which is characterized by mental deficiency, as well as a metabolic defect. F. H. Tyler suggests a sex-linked recessive type of inheritance in certain muscular dystrophies. J. W. Schut emphasizes in his paper the great variability of the hereditary ataxias which make it difficult to classify them distinctly. W. G. Lennox presents the results of his studies on 173 twin pairs subject to seizures and shows that intelligence is not significantly affected by seizures but depends on heredity, brain damage, or both. Helen Goodell presents findings suggesting the hereditary character of migraine through a recessive gene. F. J. Kallmann summarizes his position on the possibility that gene-controlled deficiencies are the basis of ill-adaptable types of personality. Bentley Glass, in his discussion of the genetic aspects of adaptability, shows that there is a virtually untapped potential in the method of co-twin control in which simple types of differences in capacities to respond are studied.

DAVID B. TYLER

GENERAL AND SYSTEMATIC BOTANY

AN INTRODUCTION TO BOTANY. *Third Edition.*

By Arthur W. Haupt. McGraw-Hill Book Company, New York, Toronto, and London. $5.50. xii + 447 pp.; ill. 1956.

THE PLANT WORLD. *A Text in College Botany. Third Edition.*

By Harry J. Fuller. Henry Holt & Company, New York. $5.50. viii + 584 pp. + 1 pl.; text ill. 1955.

COLLEGE BOTANY. *Revised Edition.*

By Harry J. Fuller and O. Tippo. Henry Holt & Company, New York. $6.90. xiv + 994 pp. + 1 chart + 1 pl.; text ill. 1954.

These three textbooks need little introduction to botanists the country over. All have been widely used, the first two for single-semester courses, the last one for full-year courses. All are competently written, profusely illustrated, and attractively bound. The emphasis in each is largely morphological, with a good deal of physiology and phylogeny thrown in. All are uniformly dull, even though, as Fuller states, an attempt was made to "steer a mid-channel course between the Scylla of pedantic minutiae and the Charybdis of superficiality." This is not enough, however, to attract students into the field of botany, and as we all know, the number of students who choose it as a major field of interest diminishes each year. In fact, among the younger scientists in biological fields there are many who have had no more training in botany than what was gained through an introductory course, and one wonders whether this is because the botany textbooks, as a group, fail to present a dynamic point of view.

Take the teaching of evolution. The principles of evolution provide the greatest unifying influence ever enunciated in the natural sciences; they have brought meaning into all areas of human thought. Yet I know of no botany text which uses these principles as a central theme to tie together all of the other subdivisions. Rather, the chapter on evolution usually occupies a subservient position, often appended to the last portion of the book like a hanging participle, and, more often than not, inadequately handled. For example, in the Fuller and Fuller and Tippo textbooks—and many sections are repeated verbatim in these two volumes—polyploidy is discussed very briefly, and the following statement is made: "Whether or not polyploidy is an important factor in the development of new species of plants and animals is a matter of dispute at present." The statement is, of course, true when it is applied to animal evolution, but hardly so for plants if one stops to consider that one half of all the ferns and flowering plants are of polyploid origin. Not only that, but by such a remark one of the most fascinating stories to come out of botanical research is lost to beginning students. When one has omitted facts such as these, together with the story of hybrid corn, the evolution of wood structure as elucidated by Bailey and his students, and the work of Jens Clausen, Goodspeed, Stebbins, Turresson, and others on the dynamics of plant populations, the heart of botany is gone. These studies can be presented to elementary students as well as to advanced ones, and all would profit. So far as the reviewer is concerned, the good botany textbook still remains to be written.

C. P. SWANSON

FUNDAMENTALS OF PLANT SCIENCE. *A Laboratory Manual.*

By G. W. Prescott and J. C. Elliott. Burgess Publishing Company, Minneapolis. $4.00 (paper). xi + 271 pp.; ill. 1955.

This is another addition to the large number of laboratory manuals printed by the Burgess Publishing Company. It was prepared for an introductory course at Michigan State University, and its usefulness and desirability will depend upon the teacher's concept of what constitutes a good laboratory in the plant sciences. This one is no better or worse than a dozen similar spiral-bound volumes. One would like to ask the authors what their Fig. 3, Plate II, on p. 31, is supposed to represent. This is the strangest drawing of a nucleus seen in many a moon.

AN INTRODUCTION TO PLANT TAXONOMY.

By George H. M. Lawrence; illustrated by Marion Ruff Sheehan. The MacMillan Company, New York. $3.25. x + 179 pp.; ill. 1955.

One-semester college classes in plant taxonomy and study groups meeting in the patios of garden clubs in the springtime have need for a short introduction to plant classification and related matters—for what amounts to a Gray's *Lessons* brought up to date. For both audiences the book must be sparkling enough to hold the reader's interest (unless the college class be considered a semi-captive audience) and factual enough to grant the user some success in identifying plants with a manual. It is a terminal course of study in nearly all instances, but now and then a person may have his interest protracted by a good text. Does Lawrence's primer fill this need? I believe it does, because (a) the organization of the subject matter is logical, (b) most of the questions that either the college sophomore or garden-club putterer may ask are answered, (c) the illustrations (in this picture-ridden world) are clear and well chosen, and (d) the invitation to read more about plants, hints for botanizing (Chapter V), self-help explanations for identifying plants (especially pp. 56–83), and possible "opportunities in taxonomy" (pp. 4–5) are clearly set forth.

Some attention is directed to the history and theory of classification, "plant structures," and nomenclature. A few pages are devoted to phylogeny and biosystematics, a summary of the development of plant taxonomy in the United States is presented, and the book concludes with a very brief sketch of 42 "important families and their characters." This last sequence of sketches aims to stress recognition marks for easy identification, but the use of italics for the most diagnostic points of each family, in the manner of Fernald's *Manual*, would have increased the usefulness of the synopses. The glossary is particularly full and useful.

It is unfortunate that the book is beset by numerous typesetter's errors and some errors of fact, particularly in Chapter VIII, where 18 mistakes of both sorts were left after the page-proof stage. Fortunately, the final

chapters on the plant families and the glossary appear to have been proofread carefully. A few factual errors may be corrected here: Prof. John Lindley was not associated with Kew, though his orchid collections and drawings ultimately passed to that great herbarium; to say that "Rafinesque left no herbarium" is a misleading statement—he left no extensive collection intact at his death, but a large number of specimens was given to Dr. William Darlington, of West Chester, Pennsylvania, by Elias Durand, who sorted over Rafinesque's effects, and another considerable series passed to the Paris Museum. Perhaps a roster of all these Rafinesque remnants and the miscellaneous sheets at the Gray Herbarium and elsewhere will be compiled by some devoted soul! Professor Jepson was born in California, so the italicized words should be added: "prior to the entrance of [Jepson] into [the] California *field of study*."

These lapses will certainly be attended to in the next printing, and since the book is fundamentally sound and useful I predict that this *Introduction* will enjoy a long text-life. "Pass the Lawrence" is already lab lingo on many campuses.

JOSEPH EWAN

A. ENGLER'S SYLLABUS DER PFLANZENFAMILIEN, *mit besonderer Berücksichtigung der Nutzpflanzen nebst einer Übersicht über die Florenreiche und Florengebiete der Erde. I. Band: Allgemeiner Teil. Bakterien bis Gymnospermen. Twelfth Edition.*

By Hans Melchior and Erich Werdermann. Gebrüder Borntraeger, Berlin-Nikolassee. DM 40. viii + 367 pp.; ill. 1954.

The present volume of this syllabus includes a general introduction to the basis and methods of systematic botany, brief discussions of the various theories dealing with the origin of parts of plants as they have arisen in the course of evolution, and a general survey of the plant kingdom from the bacteria through the gymnosperms, but not including the angiosperms. The last Engler syllabus appeared in 1936, so that the present volume takes into consideration the developments in classification which have come out of the last twenty years of study. Melchior and Werdermann have prepared the present volume, although a variety of contributors have dealt with particular divisions of the Engler system.

ILLUSTRATED GENERA OF IMPERFECT FUNGI.

By H. L. Barnett. Burgess Publishing Company, Minneapolis. $4.00 (paper). ii + 218 pp.; ill. 1955.

The author, well known in the field of mycology, has prepared an inexpensive but well illustrated and keyed manual of the Fungi Imperfecti commonly encountered in agricultural, industrial, and medicinal studies with the object of facilitating their identification. This is obviously a difficult and tedious task, but the key to the 302 illustrated genera seems eminently workable for students entering this field, or for others having but a cursory knowledge of the subject. Since this number represents less than one-third of the known genera, and since no species identifications are given, specimens difficult to track down will likely be encountered in any survey of the group, but additional references will assist the student in doubtful cases.

HANDBUCH DER SUKKULENTEN PFLANZEN. *Beschreibung und Kultur der Sukkulenten mit Ausnahme der Cactaceae. Band I: Abromeitiella bis Euphorbia.*

By Hermann Jacobsen. Gustave Fischer Verlag, Jena. DM 58. xvi + 614 pp. + 1 pl.; text ill. 1954.

This volume is apparently the first part of a comprehensive treatment of the succulent plants of the world. After a brief introduction in which the natural habitats and methods of propagation of succulent plants are considered, the remainder of the text is devoted to a detailed description of each species. The degree of comprehensiveness of the treatise is indicated by the fact that in the genus *Euphorbia* itself some 350 species, exclusive of varieties, are considered, while the text is greatly reinforced by over 500 photographs of various species in either natural or garden habitats. Where a particular genus, such as *Agave* or *Euphorbia*, has been broken down into subgenera, these are treated in a brief preface so that the relations of one species to another can be gained. The inter- and intra-generic descriptions, however, are treated alphabetically, so as to make it relatively easy to find the details for any single known species. Synonyms are also listed, with easy cross-reference data.

This series, when complete, should provide the most thorough volume available to the specialist on succulent plants. It would be a mistake to consider this a taxonomic treatise, however, since no keys are provided, and the detailed descriptions are oriented toward the horticultural rather than the systematic end. The amateur will find the volume of little use in running down an unknown species unless he has a good general knowledge of the succulents at large.

C. P. SWANSON

ORIGIN AND DISTRIBUTION OF THE BRITISH FLORA.

By J. R. Matthews. Hutchinson's University Library, London; [*Longmans, Green & Company, New York*]. Text ed. $1.80; trade ed. $2.40. 173 pp.; ill. 1955.

That the flora of any given area is of multiple origins is axiomatic. That the present composition of a flora is the result of changing times and diverse agents of dispersal and extinction is likewise basic. The British Isles are peculiarly interesting in testing this tenet, since their flora is exceptionally well known among the world's areas. Ronald Good has remarked that the story of the flora of Britain is the story of north temperate regions since the Pleistocene. In this little book Matthews, of Aberdeen University, has published 10 lectures that will provide botanists immured in the Temperate Zone with an outline of study of the origin and distribution of floras of their own areas. In short, the British Isles represent a kind of fruitfly frame of reference for phytogeography, and Matthews is the Morgan of it all.

The arrangement of the chapters is approximately chronological, with the fossil record and Pleistocene sequences comprising the first 4 chapters, followed by expansion factors, composition of the flora today, and an analysis of the "geographical elements" closing the series. The ten-point-solid typography facilitates rapid reading; maps and tables aid in quick understanding of the text, which is happily free from typographical errors. New editions of this little handbook will be called for as the tedious mapping of species' distributions nears completion in England, and as palynological data and more precise information on microclimates accumulate. Subsequent editions may profitably consider the data provided by the zoogeographers (e.g., Bryan P. Beirne, *Origin and History of the British Fauna*, 1952); certainly a consideration of the ice-free Celtic Land southwest of Britain is a concept worth serious thought, for surely plants and animals must have been living in a biome then as now.

There are roughly 1500 indigenous species in the British flora distributed among 468 to 517 genera (according to one's bases for generic limitation). The insularity of Britain is one reason for the paucity of species in its flora as a whole; the flora of France, for example, is more than three times as large as that of Britain. That 81 per cent of the total British flora consists of genera represented by only 1, 2, or 3 species "is an indication of an impoverishment brought about most probably by a barrier to re-immigration imposed by the final separation from the European mainland."

Two-thirds of Ireland is covered by plants of two distributional patterns: a "central" type of the limestone plain, and a "marginal" type of the non-calcareous hills. Ten species widespread in Britain and to be expected in Ireland are absent there. These include trees, shrubs, and herbs; *Acer campestre*, *Viburnum lantana*, and *Paris quadrifolia* are among the absentees. The fossil record not only demonstrates extinction, but also points to the diminishing range of many species following upon a general migration toward the north and west of Britain. *Astragalus alpinus*, familiar to Rocky Mountain botanists, and formerly located in Ireland, is now a relict to be found on only a few summits in Scotland. About a half dozen species have become extinct in Britain since the censuses began in the sixteenth century.

The British flora was classified into "native," "denizen," "colonist," and "alien" forms by H. C. Watson in 1847. A serious handicap in distinguishing between these types is the elementary problem of word use. A "native" is an undoubted indigenous species; a "denizen," a native of suspected introduction; a "colonist," a weedy semi-domesticate found near the habitats of man; and, finally, an "alien," a species with established introduction of more recent date. Lousley's recent revision of Watson's classification into 3 classes, "natives," "doubtful natives," and "aliens," is perhaps the best device proposed. There are 3 grades of citizenship among the aliens as proposed by Lousley, "naturalized aliens," "established aliens," and "casuals," patterned perhaps on the passport regulations set up by the Foreign Office.

Edward Forbes recognized 5 floristic elements in Great Britain in his 1846 summary of the subject, viz., the Iberian component, the Gallican element, the Kentish, the Scandinavian or Boreal, and the Germanic. The Boreal element is of particular interest to the American botanist; it is a relict coterie on the mountains of Scotland, Wales, and northwest England. The oldest of the 5 components is the Iberian; *Arbutus unedo* and *Ilex aquifolium* are examples. It is a far northern extension of a Mediterranean group. Praeger, whose several papers on the Irish flora were distilled in his *Natural History of Ireland*, concurred with Forbes' view that the Irish flora resulted from an overland immigration during postglacial periods. The southern ice-free strip in Ireland is postulated as a sanctuary for arctic plants. An Eocene palm resembling *Nipa fruticans* recalls the Tethys Sea as a factor in past movements of the British flora. The "Sequoia" of the Island of Mull may be *Metasequoia*. The flora of western Europe and the British Isles emerged during the Pliocene; sweet gale, *Myrica gale*, is "an old boreal element" in the flora. In brief, the origin of the British flora may be a mixis of European and American species. It is almost a Shavian jab to look upon the London plane as a living symbol of this mixis!

Problems still to be tackled include: (1) "altitudinal vicariism" as represented by *Armeria maritima* and *Plantago maritima*, that range from the coast to the crown of Ben Nevis (4160 ft.); (2) the intermingling of arctic and temperate species at points where they met formerly, but no longer are found together; (3) long-distance dispersal over frozen snow—the few observations at hand excite an interest in further data that may come from the high-arctic research programs in progress; and (4) the loss of native British species during the glacial advances—to judge from the full-glacial deposits, 10 species have been lost during the Pleistocene as ascertained by radiocarbon techniques. The

span of the late Pleistocene floras is now set at 20,000 years.

JOSEPH EWAN

GLOSSARY OF THE BRITISH FLORA. *Second Edition Revised and Enlarged.*

By H. Gilbert-Carter; with a preface by Charles E. Raven. Cambridge University Press, New York. $3.00. xxiv + 87 pp. 1955.

The object of this slim volume is to provide the reader with an alphabetical glossary of terms found in current editions of British flora. The meanings, pronunciations, and derivations of plant names are given, and while the principal audience addressed is British the usefulness of the book is by no means so narrowly limited. The professional as well as the amateur botanist will find much in it of etymological information, and for those who have long considered taxonomic nomenclature a hidden mystery it should greatly clarify the logicalness of the binomial system.

ITINÉRAIRES BOTANIQUES DANS L'ILE DE CUBA. *(Troisième Série). Contr. Inst. bot. Univ. Montréal, No. 68.*

By Frère Marie-Victorin and Frère Léon. Institut Botanique de l'Université de Montréal, Montréal. $2.50 (paper). 227 pp.; ill. 1956.

Jules Brunel, a student of Frère Marie-Victorin, has prepared this volume from the notes of the two authors, and it therefore represents a posthumous contribution dealing with the botanical expedition taken by the two on the island of Cuba. It is presented as a day-to-day narrative, and hence has in it a strong personal element, but in addition it deals more or less specifically with the botanical wealth with which Cuba is so fortunately blessed. The time covered is in 1940 to 1941, some three years before Frère Marie-Victorin died so tragically in an automobile accident, but the observations are still pertinent, interesting, and botanically valuable.

PLANT MORPHOLOGY

EMBRYOGENESIS IN PLANTS.

By C. W. Wardlaw. John Wiley & Sons, New York; Methuen & Company, London. $7.00. x + 381 pp.; ill. 1955.

It is startling that 1955 should be the date of the first comprehensive book on plant embryology written from the point of view of morphogenesis. Within the last 25 years, we have had several books so designated, but either they have been concerned only with certain groups of plants, or they have arrayed the material primarily for the use of the comparative morphologist. As significant as the works of Schnarf, Souéges, Maheshwari, and Johansen are, they have served the student of morphogenesis in a limited way. In this new volume by Wardlaw, we find a survey of the embryological development of all groups of plants from algae to angiosperms—only the fungi are omitted—with illustrations and discussions. The author has reported on experimental work which has been done and has invoked genetical, chemical, and physical implications and interpretations when he considers them pertinent. He also has introduced evidence for evolutionary theory and phylogenetic sequence when relevant.

The book consists of 16 chapters, with 84 illustrations and an excellent bibliographic coverage of 738 references. There are occasional omissions of expected references, but this is not surprising in an initial compilation of reference material in any field.

Chapter 1 is given the title of Scope and Outlook; in its 5 pages, Wardlaw introduces problems of morphogenesis at the fundamental level. Among these he calls attention to the 2 viable cells in the life history of many plants, the spore and the fertilized egg. He points out that though both give germs, the former produces a somewhat indefinitely shaped organism called a prothallus, while the latter is an embryo with a definite developmental pattern. Both viable cells have the same genome and yet are strikingly different. Why are they different? Evidence suggests that haploidy and diploidy are not enough to account for the differences. Wardlaw refers to Ward's initial venture toward a study of the problem; but the experiments for the solution of even this fundamental contrast in development have not yet been made.

In this chapter, Wardlaw also emphasizes the extent to which students of morphogenesis have often "pressed into service . . . the perennially embryonic shoot apex as a substitute for the young embryo." This he suggests is due to the "manipulative difficulties which the latter presents." He points out that the meristematic apex and the developing zygote "are not strictly comparable," a point too often neglected. "Studies of embryogenesis . . . not only carry their own interest; they should be regarded as the primary approach to morphogenesis." He further calls attention to the outstanding and ingenious studies in the experimental embryology of animals and to the biochemical relationships established in consequence. He warns, however, that the problems of the zoologist are peculiarly different, though the technical procedures and even the stimulation they offer may be helpful to the botanist. With the problems of cellulose walls in plants, such warnings are well advised.

Chapters 2 to 15 are devoted to the several major groups of living plants, cover existing knowledge on various aspects of embryology, and suggest new problems. The groups in turn are: the Algae; Bryophyta; Psilotales and Equisetales; Lycopodineae, including *Lycopodium* and *Phylloglossum*, *Selaginella* and *Isoetes*;

the Eusporangiate Ferns; the Leptosporangiate Ferns; the Gymnosperms—Cycadales and Ginkgoales, Coniferales and "Gnetales"; and the Flowering Plants. The last are broadly considered from the embryogenic point of view in 3 chapters, with excellent coverage of the treatments of Maheshwari, Johansen, and Souéges and their students as well as those of many others. One chapter deals with special subjects, such as the embryogenesis of primitive angiosperms as suggested by Bailey, Swamy, and coworkers; the embryos of more advanced dicotyledons and of monocotyledons; the embryology of parasites and saprophytes; apomixis; and polyembryony. A chapter is given to experimental and analytical studies carried out on angiosperms, an interesting chapter that includes work done and problems raised by the culture in vitro of embryos.

The final chapter, entitled General Conclusions, is a fitting discussion of the problems raised. Emphasis is laid upon the difficulty of using evidence from embryogenesis in taxonomic and phylogenetic studies in a well-documented treatment.

This book will be an indispensable possession for workers in morphogenesis. It should prove valuable to those in comparative morphology, agronomy, horticulture, floriculture, and many other fields. It will undoubtedly stimulate new efforts in plant embryogenesis.

RALPH H. WETMORE

PRAKTISCHE EINFUHRUNG IN DIE PFLANZENMORPHOLOGIE. *Ein Hülfsbuch für den botanischen Unterricht und für das Selbststudium. Erster Teil:* DER VEGETATIVE AUFBAU.

By Wilhelm Troll. Gustav Fischer Verlag, Jena. DM 15.60. ix + 258 pp.; ill. 1954.

This is a well-illustrated and thorough volume on plant morphology which is slanted toward the practical side. The morphology considered is restricted to the seed plants, and includes all organs from the seed and embryo to the mature plant. Particular emphasis is given to that morphology which would be of interest to propagators, but the text in general is not so narrowly conceived. It should be an excellent volume for those students who handle the German language with facility.

PLANT PHYSIOLOGY

AN INTRODUCTION TO PLANT PHYSIOLOGY. *Fifth Edition.*

By W. O. James. Clarendon Press, Oxford. 17s. 6d. viii + 303 pp. + 1 pl.; text ill. 1955.

This is an excellent introductory textbook of plant physiology. Since the appearance of the first edition in 1931, the book has had considerable popularity, especially in England, where the author is a well-known plant physiologist. The present edition gives a balanced discussion of the current problems in plant physiology. It is written on an elementary level, but the material is well organized and interesting. The following subjects are covered: material and energy (photosynthesis, chemosynthesis), sugars and the plant materials formed from them, nitrogenous compounds, the decomposition of sugars and the liberation of energy, some physical characters of organic substances, catalytic protoplasm (enzymes), water, nutrition, growth, and irritability.

Each chapter is concluded with an experimental section. The experiments are closely related to the material discussed in that chapter and are of great help in tying the work together. The experiments are carefully described and require a minimum of equipment.

This text probably can best be utilized for the second half of a general botany course. It is of a consistently higher caliber than the sections on plant physiology one finds in general botany books.

G. R. NOGGLE

LABORATORY PLANT PHYSIOLOGY. *Third Edition.*

By Bernard S. Meyer, Donald B. Anderson, and Carroll A. Swanson. D. Van Nostrand Company, Toronto, New York, and London. $3.50 (paper). viii + 168 pp.; ill. 1955.

Now in its third edition, this laboratory manual is familiar to those who make use of the textbook written by the two senior authors. The experiments outlined are those which can be carried out by relatively untrained students, provided care and diligence are exercised, and they give, in total aspect, a reasonably complete introduction to that aspect of plant physiology which is comprehensible to the beginning student. The experiments are carefully selected, the procedures well explained, and the objectives kept well in mind.

HORTICULTURAL SCIENCE. *A Reading and Laboratory Manual with Discussion Questions.*

By Gordon T. Nightingale; edited by Norman F. Childers. Horticultural Publications, Rutgers University, New Brunswick. $2.50. viii + 111 pp.; ill. 1955.

Horticultural Science is not, as the title might seem to indicate, a textbook of horticulture, but rather a laboratory manual of plant physiology as related to the practice of horticulture. Plants themselves are studied instead of descriptions of plants or theories regarding them. The students are given a working hypothesis and, after conducting their experiments and noting the responses of the plants under observation, they are in a position to accept or discard the hypothesis.

Throughout the book the basic point of reference is the carbohydrate-protein relationship. Problems of

vegetative development, the degree of fruitfulness, nutrition, the influence of environmental factors, and other physiological processes are all correlated with this relationship. In each chapter a varying amount of pertinent general information is given, followed by the laboratory exercises to be performed, a list of discussion questions and selected references.

The individual chapters deal with: Protein-Carbohydrate Relationships; Sand Culture Technique; Photoperiodism; Nitrate Assimilation; Ammonia and Urea; Protein Synthesis and Reutilization; Photoperiod, Temperature, Light and Other Factors; Carbohydrates; Nutrient and Soil Solutions; Oxygen Deficiency; Temperature Effects; Ion Interrelations; Macro-Nutrient Deficiencies; Micro-Nutrient Deficiencies; and the Field Log, a method of obtaining a quantitative audit of any growing crop.

ALBERT F. HILL

ECONOMIC BOTANY

AMERICAN AGRICULTURE. *Its Structure and Place in the Economy. A Volume in the Census Monograph Series, for the Social Science Research Council in cooperation with the U. S. Dept. of Agriculture, Agricultural Research Service and the U. S. Dept. of Commerce, Bureau of the Census.*

By Ronald L. Mighell. John Wiley & Sons, New York; Chapman & Hall, London. $5.00. xii + 187 pp.; ill. 1955.

This is the first volume in the new Census Monograph Series, designed to interpret and evaluate the mass of detailed information and the agricultural statistics compiled by the Bureau of the Census. It presents in simple, direct language the midcentury status of American agriculture, its structure, its constituent parts and how they are assembled, and tells how agriculture as a whole fits into the economy of the Nation.

With the farm problem so much in the public eye, the average reader will welcome this book which gives in such non-technical language an accurate picture of modern farms and farm people, and the background necessary for an intelligent understanding of the various policies and programs which are constantly being advocated by one group or another.

Many topics are considered, among them the changes in agricultural production and organization over the last half century or so, the present role and significance of agriculture, the great variation in size and type of the nation's farms, the social and family characteristics of agriculture, the importance of technology in modern farming, cooperatives and other group interests in agriculture, and rural sociology. Distinction is made throughout between commercial and non-commercial farms. Many figures and tables supply the necessary statistical data. The publications of the 1950 Census of Agriculture and a short bibliography are included in the appendix.

ALBERT F. HILL

THE AGRICULTURAL REGIONS OF THE UNITED STATES. *By Ladd Haystead and Gilbert C. Fite. University of Oklahoma Press, Norman.* $4.00. xx + 288 pp. + 24 pl. + 2 folded maps; text ill. 1955.

This most readable book presents accurate and up-to-date information regarding American agriculture and the American farmer and his way of life. Its goal is primarily the purely functional one of furnishing an exhaustive analysis of the many different types of agriculture, and of the economic and geographic factors responsible for making this country's agricultural production one of the most varied in the world. In addition, however, there is abundant evidence of the emotional factors involved, the drama and beauty of farm life and the quiet interrelationship between the farmer and his environment, which is only possible in a country where "no man farms because he has to," but only because "he wants to," and in so doing finds his life's fulfillment in this "marriage to the soil."

The first chapter deals with matters of general interest. The authors explain why so few people have a really adequate picture of American farming and why we have so much inept agricultural legislation. Geographical and historical reasons (particularly the constantly changing crops and farm patterns) are responsible for most of the misconceptions. Haystead and Fite also point out that, because of the unique nature and extreme variability of our agriculture, there is no such thing as a typical American farm or a typical American farmer. There is, however, one typically American farm product—corn. The great need for conservation is stressed, and the chapter closes with a comparison with Soviet agriculture.

The balance of the book is devoted to a detailed account of the 11 agricultural regions of the United States, designated as follows: New England: Land of Abandonment; The Middle Atlantic States: Prime Market; Delmarva [Delaware, Maryland, Tidewater Virginia]: Intensive Region; The Middle South: Reorganizing Belt; The Deep South: The Shifting Cotton Belt; The Corn-Soy Belt: Feedbag of Democracy; The North North-Central Region [Michigan, Wisconsin, Minnesota]: Dairyland; The Western Prairies and High Plains: American Granary; The Southwest: Far Distances and Dry Skies; The Rocky Mountain States: A Farm in the Sky; The Western Slope: Land of Tomorrow.

Among the many topics considered may be mentioned: the values of farm lands and the factors which determine buying and selling prices; the areas best suited, or unsuited, for certain types of farming, now and in the future; shifts in agricultural production;

crop and livestock production by areas and states: their size, value, dependability for income, and marketing; new techniques and equipment, current problems and how to solve them. In the final chapter, Tomorrow's Heritage, predictions are made for the future of our unbelievably dynamic agriculture.

The book is illustrated with many maps and charts, tables and photographs. There is a short bibliography, with notes on sources and additional reading, and an adequate index.

ALBERT F. HILL

GRAIN CROPS. *Second Edition.*

By Harold K. Wilson. McGraw-Hill Book Company, New York, Toronto, and London. $6.50. x + 396 pp.; ill. 1955.

The second edition of this valuable work incorporates much of the data that has become available since the first edition was published in 1948. A new chapter dealing with soils, soil fertility, and the interrelationship between soils and grain crops has also been added. Many new illustrations have been selected, and the references and review questions at the end of each chapter have been revised and brought up to date.

The book is a complete guide, with concise factual information concerning all phases of the subject of grain crops ranging from the broad fundamental principles underlying their culture, harvesting, storage, marketing, and improvement to more detailed accounts of individual crops. These include not only the major cereals, but flax, buckwheat, soybeans, millets, and sorghums as well. Four chapters are devoted to corn, the most important American crop plant.

ALBERT F. HILL

COCOA.

By D. H. Urquhart. Longmans, Green & Company, London, New York, and Toronto. $7.00. xx + 230 pp. + 56 pl.; text ill. 1955.

Inasmuch as cocoa is such an important plant in world economy, it is surprising that no comprehensive book on the subject has appeared since 1932. The need for such a work has long been obvious, so the present volume will be welcome. Written by a man with wide experience in cocoa-growing, it includes chapters by other authorities as well. Although designed primarily as a handbook for cocoa planters, the subject matter should be of interest to all students of tropical agriculture.

Since *Cocoa* is the second in a series of books devoted to tropical agriculture sponsored by the Colonial Advisory Council of Agriculture, Animal Health, and Forestry, it is not surprising that the chief emphasis is on British West Africa, although reference is made throughout to other parts of the world. This emphasis is perhaps justified by the fact that today Africa produces 65% of the world's cocoa, as compared with 34% for America and 1% for Asia. Nevertheless, one wishes that more specific information might have been given in regard to conditions in Brazil, the third largest producer of cocoa, and in other countries of the Western Hemisphere.

A much too short introductory chapter treats of the history of cocoa, its origin, the development of new sources of supply, the increase in consumption, and the accompanying expansion of production, and gives an outlook for the future.

The botany of cocoa (Chapter II) is likewise discussed exceedingly briefly. Consequently, much pertinent material of general interest has been omitted or is merely referred to without adequate explanation. The great variability in pod shape, and the hybrid origin of modern cocoa, for example, might well have been considered in greater detail.

The balance of the book, however, presents a wealth of information regarding the cocoa industry. Separate chapters are devoted to climate, soils, pests and diseases, virus diseases, and cocoa grades and grading. Four chapters are concerned with cocoa plantations and consider respectively: establishment and maintenance; propagation; planting and after care; and preparation for market. The important native cocoa culture in West Africa is discussed, as well as the rehabilitation of the cocoa plantations in Trinidad, and the more important aspects of cocoa research. The final chapter on world production and consumption presents valuable data, statistics, and charts regarding cocoa and cocoa products.

There are several appendices, which furnish information especially useful for planters, a glossary, and an unusually complete index. The book is well illustrated with over a hundred photographs, some in natural color.

ALBERT F. HILL

PRACTICAL HORTICULTURE.

By James S. Shoemaker and Benjamin J. E. Teskey. John Wiley & Sons, New York; Chapman & Hall, London. $4.20. viii + 374 pp.; ill. 1955.

This book, one of the Wiley Farm Series, is designed primarily for students of vocational agriculture, farmers, and gardeners. It presents the latest information on a wide range of subjects covering the entire horticultural field in a simple, concise, yet entirely adequate manner. The instructions and advice given are so practical and so easy to follow that all garden enthusiasts, whether interested in ornamental shrubs, flowers, vegetables, fruits, or even lawns, should be able to achieve greater success in their efforts. Comprehensive information is furnished regarding varieties, propaga-

tion, planting, pruning, weed and disease control, harvesting, storage, and general culture of a wide range of plants. Brief suggestions for further reading are given at the end of each chapter.

ALBERT F. HILL

PROPAGATION OF HORTICULTURAL PLANTS. *Second Edition.*

By Guy W. Adriance and Fred R. Brison. McGraw-Hill Book Company, New York, Toronto, and London. $6.50. x + 298 pp.; ill. 1955.

This second edition of *Propagation of Horticultural Plants* includes much new material, dealing especially with seed treatments that hasten germination, soil sterilization, costs of growing plants in different kinds of hot beds, factors that influence propagation by cutting, layerage, budding and grafting, the use of hormones, present-day propagation schedules, and stock and scion combinations. A chapter on pruning has also been added.

The book gives an excellent idea of horticultural practices and techniques in general and the various methods of propagation in particular. The relationship of these to the anatomy and physiology of the plants themselves is emphasized. A list of questions and suggested references is appended to each chapter. The clarity of presentation should make the work equally valuable to amateur gardeners, commercial growers, and students of horticulture.

ALBERT F. HILL

PLANT PROPAGATION *and Garden Practice. A Practical Guide to the Various Methods of Propagating Trees and Shrubs, Herbaceous Plants, Fruits and Vegetables.*

By R. C. M. Wright; with preface by T. J. Walsh. Criterion Books, New York. $4.50. 192 pp. + 23 pl. 1956.

All but the expert gardener can read this book with profit. In quite forthright terms, it takes the reader through all stages and types of propagation from the harvesting of seed to the handling of seedlings, from cuttings of all types to layering, grafting, and budding, and from annual herbs to longest-lived trees. More than 100 photographs nicely supplement the text, and will provide visual directions for many of the more complicated types of propagation. Wright probably had a British audience in mind when he wrote this book, but plants, fortunately, recognize no national boundaries, so that the American gardener can share the secrets of his British counterpart.

VEGETABLE GROWING. *Fifth Edition, Thoroughly Revised.*

By James Edward Knott. Lea & Febiger, Philadelphia. $5.00. 358 pp.; ill. 1955.

The techniques of commercial vegetable growing change so rapidly because of the introduction of new types of labor-saving machines and the resultant alterations in planting methods and varieties used, and because of the utilization of new methods and materials for weed and disease control, that it is difficult for the grower to keep abreast of all the recent developments. Hence the need for a revision of this standard work within six years after the fourth edition was published (*Q.R.B.*, 24: 241. 1949).

Nearly one-half of the present book is devoted to general considerations of the underlying principles and horticultural practices involved in vegetable growing. The individual crops are then discussed under the main headings of cool-season and warm-season crops. Since some seventy are included, the treatment in each case is of necessity rather limited, but the essential facts are adequately presented. Furthermore, a selected list of publications for additional reading is appended to each chapter, and an unusually extensive index has been compiled.

ALBERT F. HILL

GARDEN SPICE AND WILD POT-HERBS.

By Walter Conrad Muenscher and Myron Arthur Rice; with illustrations cut on wood by Elfriede Abbe. Comstock Publishing Associates, a division of Cornell University Press, Ithaca. $5.75. x + 211 pp.; ill. 1955.

When a high-priced limited edition of this beautiful book appeared early in 1955, it was hailed, not only as a welcome addition to our herb literature, but also as an outstanding example of modern techniques in the field of graphic arts.

Now that a popular-priced edition is available, it should appeal to all botanists, whether amateur or professional, and to everyone interested in the fine art of illustrating as well. The product of close collaboration between a trained botanist, a horticultural expert, and a talented botanical artist, *Garden Spice and Wild Pot-Herbs* is in many respects reminiscent of the old herbals.

The 56 full-page plates, with two exceptions, are made from wood engravings and are not only correct as to detail, but because of their great beauty, greatly enhance the value of the work. A plate depicting herb seeds is of special interest. There are also many text figures, likewise made from wood-cuts.

From a botanical standpoint the book is most useful. The senior author has long been an authority on herbs. In consequence, the descriptions of the plants and the comments regarding their culture and uses are scientifically accurate, a condition not always encountered

in herb books. In the Introduction, much valuable general information is given, particularly in regard to the culinary herbs. Part I is concerned primarily with the herbs used in cookery, arranged according to families. Part II treats of methods of propagation, hardiness, harvesting, drying, and storing. In Part III, 18 wild pot herbs are considered, many of them with highly desirable qualities, but usually overlooked through lack of familiarity. Part IV is devoted to the classification and nomenclature of plants, and has a list of synonyms. There is also a glossary and an index.

ALBERT F. HILL

CHERRIES AND CHERRY PRODUCTS.

By Roy E. Marshall. Interscience Publishers, New York and London. $8.50. xiv + 283 pp.; ill. 1954.

This volume, the fifth in a series dealing with the chemistry, physiology, and technology of food and food products, is concerned with cherries and cherry products. Written by an expert with long experience in the field, it furnishes a wealth of information regarding present-day horticultural practices and processing aspects of the industry.

The various topics discussed include: the history of the cherry; the cherry industry; species, groups, and varieties; the development and structure of the fruit; environmental and management factors affecting cherry production; diseases, pests, and their control; chemical and physiological changes in the fruits; fruit maturity and handling; factors affecting the post-harvest life of the cherry; pretransit treatment and transportation; canning; freezing; brined cherries and their products; cherry juice; dehydration; and standards and grades. A list of selected references is appended to each chapter, and there are both subject and author indices.

ALBERT F. HILL

OUR NATIONAL FORESTS.

By Bernard Frank; foreword by Justice William O. Douglas. University of Oklahoma Press, Norman. $4.00. xx + 238 pp. + 32 pl.; text ill. 1955.

For the approximately 165 million owners of the National Forests, i.e., for all citizens of the United States, this book provides a highly informative, unofficial, and non-technical report on the manner in which their forests are managed and what they have to offer. The author, an assistant chief of the United States Forest Service (Department of Agriculture), is not only technically well qualified to undertake such a chore, but he possesses the rather unusual faculty of writing accurately in a popular style. The basic policy of "multiple use" under which the National Forests are administered is given primary emphasis, as are the associated and inevitable difficulties that arise in a democracy through the continuing efforts of unthinking pressure groups and special interests to exploit the wealth of the forests without thought of future needs. An appendix, summarizing National Forest recreational and scientific resources, will be found of special interest to field biologists. The National Forests are listed by states, and for each there are shown: the forest and district ranger headquarters, scenic attractions, wildlife present, water (streams, lakes, etc.), location of camp and picnic areas, and special features of the area. The book is illustrated by about 50 half-tone photographs, all of which are above average and some of which are excellent. For all those who go to the forests, whatever the reason, this book will prove of continuing interest and value as a reference.

SCOTT S. PAULEY

FORESTRY HANDBOOK.

By Reginald D. Forbes; staff editor, Arthur B. Meyer; edited for the Society of American Foresters. The Ronald Press Company, New York. $15.00. xii + 1062 pp.; ill. 1955.

Under the sponsorship of the Society of American Foresters, work on this publication was initiated in 1949 by an editorial committee and 19 subject-matter committees, totaling more than 100 individuals. Final editing for publication was done by R. D. Forbes and A. B. Meyer. The *Handbook* is designed primarily for use as a reference by foresters and workers in related fields. It brings together various formulas, converting factors, working methods, and techniques employed in forestry as well as in such associated specialty fields as wildlife, range, and watershed management. The material is organized under 23 subject headings (e.g., silvics and silviculture, economics and finance, insects and diseases, recreation, yield tables and stocking, etc.), each of which is preceded by a detailed table of contents. About 300 line drawings, graphs, charts, and half-tones have been used to advantage for illustrative purposes and as a means of condensing much technical information and data. Condensation has also been effected by the liberal use of tables (ca. 400). Indexing of the volume appears to have been done in sufficient detail to insure prompt location of desired material. Foresters and workers in related fields, particularly those isolated from adequate library facilities, will find the *Handbook* a work of considerable practical usefulness.

SCOTT S. PAULEY

TIMBER IN YOUR LIFE.

By Arthur H. Carhart; introduction by Bernard DeVoto. J. B. Lippincott Company, Philadelphia and New York. $4.00. 317 pp. 1955.

Timber in Your Life is a well-organized, non-technical book designed to acquaint the average citizen with American forest problems, past and present. In spite of the tenor of DeVoto's introduction and the jacket blurb's implication that Arthur Carhart employs "shock" tactics, the reader will find that the author's style is calm and his arguments logical. Carhart has had long experience in forestry and conservation movements, and his book is threaded with personal reminiscences and anecdotes which contribute much to the book's readability. For those unfamiliar with the sad early history of profligate forest exploitation in this country, the book will be read with profit. Such readers will also be reassured to learn that, as the author points out, something has been, and is being, done about it.

SCOTT S. PAULEY

THE PLANT QUARANTINE PROBLEM. *A General Review of the Biological, Legal, Administrative and Public Relations of Plant Quarantines with special Reference to the United States Situation. Ann. cryptog. Phytop. (incorporating Ann. bryol., Hague) Vol. XI, edited by Frans Verdoorn.*

By W. A. McCubbin. Ejnar Munksgaard, Copenhagen. D. Kr. 32.—. 256 pp. 1954.

As the subtitle indicates, this book is a general account of all phases of the plant quarantine problem. Although written with special reference to the situation in the United States, the general considerations and conclusions are international in scope and should be of worldwide interest.

The author, with thirty years of experience to draw upon, presents the subject as a composite whole and brings together information on the various aspects in sufficient detail to make clear the fundamental features and relations of plant quarantines. The resulting summarization, presented in simple language with explanations and comments when necessary, should give the reader an adequate grasp of the problem. Although the subject matter may perhaps be familiar to plant quarantine officials, it will be welcomed by all others who, on occasion, have to interpret, appraise, or counsel regarding quarantine matters. The average citizen, moreover, whose life is often affected by the impact of the many regulations, will surely appreciate this clear and lucid explanation of the "whys and wherefores" of these measures.

The several chapters are entitled: Introduction; The Biological Background; Social and Economic Relations; Legal Features; Administrative Aspects; A Current Problem Examined; Appraisal and Outlook; and The World Situation. A summary of Federal Plant Disease Quarantines is given in an appendix, and there is also a bibliography and a short index.

ALBERT F. HILL

GENERAL AND SYSTEMATIC ZOOLOGY

ELEMENTS OF ZOOLOGY. *McGraw-Hill Publications in the Zoological Sciences.*

By Tracey I. Storer and Robert L. Usinger. McGraw-Hill Book Company, New York, Toronto, and London. $5.50. xii + 552 pp.; ill. 1955.

Storer's larger *General Zoology* is well-known and has been previously noted in these pages (*Q.R.B.*, 28: 73. 1953). Although the present volume has a junior author and goes by a different title, this review will limit itself to a comparison with the earlier textbooks by the senior author. The most noticeable change is the reduction in material, about 300 pages having been eliminated. About the only alteration in the sequence of presentation has been to shift the History of Zoology from the middle to the beginning. The second most important change has been a shift in emphasis from the phylogenetic survey of the animals to more material on the anatomy and physiology of man. In the previous texts the ratio was about 23 per cent, but it is now closer to 37 per cent. This of course means that the chapters on the phyla have been condensed. Some of the more detailed illustrations have been omitted. Quiz and review questions have been substituted for references at the ends of the chapters. As a result of these changes a certain amount of factual material is missing, but as compensation the coverage is better balanced and will fit better a two-semester course whose students are taking zoology for a variety of reasons. However, the essential character of the book is unchanged. It contains an abundance of material often presented with a sparingness of words, and it is still generously illustrated. We predict a wider reception than ever for this new revision.

HENRI C. SEIBERT

ZOOLOGY. *Second Edition.*

By A. M. Winchester and Harvey B. Lovell. D. Van Nostrand Company, Toronto, New York, and London. $6.25. viii + 582 pp.; ill. 1955.

The second edition of this textbook has been revised and greatly improved. More physiology has been added, other chapters expanded, and previous errors corrected (cf. *Q.R.B.*, 24: 150. 1949). A junior author, who has been responsible for the revision of several chapters, has been added. A better balance of subject material has been achieved. The photographs, which form the greater proportion of the illustrations, have evidently been selected with care, as they show the desired information very well. With so many zoology textbooks now available to choose from, it would be quite a task to make minute comparisons and to point out all good and bad features of any one. It should be mentioned, however, that the style of writing is here non-technical, but unfortunately often to the point of juvenility, as in the

case of discussing the activities of the "mother bird" and "father bird." This style may then lead to such statements as that the "parent seems proud of the offspring," when referring to a bird. Apart from such lapses, the book is sound.

HENRI C. SEIBERT

LABORATORY STUDIES IN THE PRINCIPLES OF ZOOLOGY. *By J. M. Carpenter, J. M. Edney, and R. W. Barbour. Burgess Publishing Company, Minneapolis.* $2.00 (paper). viii + 71 pp.; ill. 1955.

The chief claim to distinction of this laboratory guide lies in the fact that a formerly 2-semester course in zoology has been reorganized into a 1-semester course. A careful choice of material was necessary in the limited time, and so 8 basic exercises were selected to cover the more important topics. These are: Taxonomy (covers 10 phyla, the classes of the Chordata, orders of the Amphibia, families, genera, and species of the local Salientia); Use of the Compound Microscope; The Cell; Cell Division (*Ascaris*); Ontogeny (*Ascaris* and frog); Phylogeny (paramecium, sponge, hydra, and earthworm); Homology (limbs of frog, bird, horse, and man); and Morphology (frog).

HENRI C. SEIBERT

LABORATORY EXPLORATIONS IN GENERAL ZOOLOGY. *Third Edition.*

By Karl A. Stiles. The MacMillan Company, New York. $3.75. xii + 291 pp.; ill. 1955.

The third edition of this laboratory guide has been expanded to include dissection of animals previously excluded. The general theme is a survey of the animal kingdom; all important phyla are represented and some of the larger phyla have several examples. Outline drawings are provided for labeling. The instructions are printed (on both sides of the page) and include lists of additional suggested drawings, possible demonstrations, questions and problems to be solved. The sheets can be easily torn out and are already punched for loose-leaf notebooks. Additional exercises on the cell, the microscope, heredity, and embryology, although brief, are also present, as well as one for a field trip.

HENRI C. SEIBERT

TAXONOMIC KEYS *to the Common Animals of the North Central States Exclusive of the Parasitic Worms, Insects and Birds.*

By Samuel Eddy and A. C. Hodson. Burgess Publishing Company, Minneapolis. $3.00 (paper). x + 141 pp.; ill. 1955.

This book is perhaps one of the best of its nature that I have run across. For some of the groups the keys are as good as can be had. The illustrations are accurate, plentiful, and most of them help define some of the characters utilized in the keys. It must be remembered that the purpose of this publication is to help students identify animals with reasonable accuracy. It is not intended for specialists, nor to supplement standard texts, nor should any identifications based on it be accepted without verification. I think a warning to that effect would have been appropriate in the Introduction. Uncommon species are omitted. There are a few typographical slips such as are bound to occur whenever so many scientific names and terms are used. I would recommend that all students of natural history in the North Central area have a copy available.

HENRI C. SEIBERT

A REVISION OF THE SUBORDER MESOSTIGMATA (ACARINA), BASED ON NEW INTERPRETATIONS OF COMPARATIVE MORPHOLOGICAL DATA. *Spec. Pub. No. 11.*

By Joseph H. Camin and Flora E. Gorirossi. The Chicago Academy of Sciences, Chicago. $1.50 (paper). 70 pp.; ill. 1955.

A REVISION OF THE CHIGGERS OF THE SUBGENUS *GAHRLIEPIA* (ACARINA: TROMBICULIDAE). *Smithson. misc. Coll., Vol. 128, No. 6. Pub. 4215.*

By Robert Traub and Mary Lou Morrow. Smithsonian Institution, Washington. $0.00 (paper). iv + 89 pp.; ill. 1955.

These publications are outstanding contributions to an understanding of the systematics of the Acarina or mites. They exemplify the type of approach that must be made if steady, sure progress is to be gained in arriving at a definitive classification of the group. One paper is concerned with the revision of a suborder, while the other is a revision of a subgenus. Both papers, however, have much in common. New fundamental data have been presented by both. The techniques of comparative anatomy have been used in both. Both studies have emphasized the importance of homology in interpreting results. Excellent keys, illustrations, and diagnoses characterize all of the taxonomic entities.

Camin and Gorirossi have divided the Suborder Mesostigmata into 2 supercohorts, the Monogynaspida and Trigynaspida. In so doing, they have solved a number of perplexing problems that have confronted students of these mites for the past fifty years.

Traub and Morrow have more than doubled the number of known species in the subgenus *Gahrliepia*. Eighteen of the 33 included species are new. The members of the subgenus have been found only in the Orient and Africa, particularly in the primary forest. They are parasitic on mammals and they apparently have a higher degree of host specificity than most other larval trombiculids. Many of the species attach themselves

singly on their hosts rather than in clumps. Furthermore, species of *Gabrliepia* imbed themselves more deeply in the skin than most trombiculids, and one species, *G. penetrans*, is found completely buried in the skin of the perineum of its host.

G. W. WHARTON

CATALOGUE OF THE TYPE SPECIMENS OF MICROLEPIDOPTERA IN THE BRITISH MUSEUM (NATURAL HISTORY) DESCRIBED BY EDWARD MEYRICK. *Volume I.*

By J. F. Gates Clarke. British Museum (Natural History), London. £3. viii + 332 pp. + 4 pl. 1955.

CATALOGUE OF THE TYPE SPECIMENS OF MICROLEPIDOPTERA IN THE BRITISH MUSEUM (NATURAL HISTORY) DESCRIBED BY EDWARD MEYRICK. *Volume II. Stenomidae, Xyloryctidae, Copromorphidae.*

By J. F. Gates Clarke. British Museum (Natural History), London. £6. iv + 531 pp.; ill. 1955.

Edward Meyrick, an amateur English lepidopterist who died in 1938, was the author of at least 14,199 nominate species of Microlepidoptera, exclusive of most of the Pyralidoidea. This puts him well up among the leaders of the great namers in zoology. He thus named a large proportion, in some cases a majority, of the known species of these moths in the Indo-Australian, Ethiopian, and Neotropical regions, as well as many in the Holarctic Region. His publication extended from 1875 to 1939. He abhorred the use of the all-important genitalia as taxonomic characters; relied almost entirely upon wing venation, color, pattern, and various external characteristics; refused to recognize the value of types (this in a group where they are as important as in any); labeled his specimens inadequately and inconsistently; and in general did most of the things that give modern systematists the horrors. I cannot help thinking of him as the last of the great robber barons of systematic zoology.

Although many of his types are in various institutions (many cannot be located or identified), the majority are now in the British Museum (Natural History). Fortunately the task of selecting, designating, and establishing these types and of stabilizing the enormous nomenclature involved was promptly undertaken as a joint project of the British Museum (Natural History), the Bureau of Entomology and Plant Quarantine of the U. S. Department of Agriculture, and the Smithsonian Institution, and was entrusted to Dr. Clarke of the Bureau of Entomology and Plant Quarantine. These volumes are the first two of at least six volumes prepared by J. F. G. Clarke after what is certainly one of the largest and most complex, painstaking, and essential studies in modern systematics. This work will prevent utter nomenclatorial confusion in one of the major groups of animals, and will save untold labor and error for future generations. Many of the taxa involved being of great economic importance, the work is of major significance for applied entomologists as well as for workers in "pure" systematics.

The chief item of Volume I is a catalog of all known Meyrick trivial names. In this are given for each name the genus, the reference to the original description, and the present location (if known) of the type. The type species of genera are indicated. This volume also contains an historical sketch of Meyrick's activities, a list of his catalogs and other papers dealing with his collection, an illustrated discussion of his labeling habits, and an invaluable discussion of his ideas of classification in the light of modern knowledge and techniques.

Volume II covers the families Stenomidae, Xyloryctidae, and Copromorphidae. For every nominate species studied by Clarke there are given the reference to the original description, the type designation (when necessary) and the labeling of the type, a diagnosis of the characters as published by Meyrick, a photograph of the pattern and other external characters of the type, and photographs of the genitalia of the type as dissected by Clarke. Figures are also given of the venation, and there are photographs of the other important structures for the type species of genera named by Meyrick. The illustrations are excellent.

To the general biologist and to all who are not working in the Microlepidoptera, these and the subsequent volumes will have little direct interest. They are, however, of monumental importance in the systematics of the taxa concerned in all parts of the world. They will remain for a very long time as outstanding examples of international cooperation and of the highest level of modern systematics.

ALEXANDER B. KLOTS

AN ANNOTATED LIST OF THE MOTHS OF MICHIGAN EXCLUSIVE OF TINEOIDEA (LEPIDOPTERA). *Misc. Publ. Mus. Zool. Univ. Mich., No. 88.*

By Sherman Moore. Museum of Zoology, University of Michigan, Ann Arbor. Free upon request (paper). ii + 88 pp. 1955.

A total of 1066 "forms" (as the author says) of the Macrolepidoptera are listed as having been authentically recorded from Michigan. The known records of each are given by counties, with inclusive dates. References are also given, when possible, to illustrations in the widely circulated Holland's *Moth Book* (1905). The list is based on the specimens in the collection of the University of Michigan and in 3 private collections.

The author very properly appears to have taken at least some precautions against misidentifications (the greatest bugaboo of such lists) by submitting the material (for something less than 10 per cent of the determinations) to a number of authorities, not all of whom are fully competent by modern standards, although some of them are above question. Reference is

made to the first and second parts of W. T. M. Forbes' *Lepidoptera of New York and Neighboring States*, but not to the third part of this publication (published in July, 1954), which is completely indispensable for the enormous family Noctuidae, because of the unavailability of that part when the list was being prepared.

An exceedingly grave fault, which will very largely negate the value of this list for scientific work, is that it follows the archaic taxonomy and nomenclature of the 1917 *Check List* of Barnes & McDunnough instead of the present standard, the 1938–39 McDunnough list. The excuses for doing so are that the University of Michigan collection follows the 1917 list, and that the cost of relabeling and rearranging the collection would "probably not be justified, for still another list may be expected before many years." The arrangement of the University collection hardly seems an adequate reason for the perpetration of its thoroughly outmoded systematics upon the public; and the statement that another check list may be expected before many years is almost certainly unwarranted. The 1917 list was compiled at a time when taxonomy was in a comparatively primitive condition, so that many of its so-called species have since been shown to be either synonyms, subspecies, or complexes consisting of more than one species. Identifications which follow such a taxonomy are, therefore, often almost meaningless today. In the higher categories the title of this list is a good example of this. In 1917 the term "*Tineoidea*" included all of the so-called "Microlepidoptera"; today it is restricted to one superfamily containing (*teste* Forbes) 7 families. By any standard since 1920, therefore, this list should include all of the Suborder Jugatae (Homoneura) and 8 superfamilies of the Frenatae (Heteroneura), containing 35 families.

I do not see how any serious student can use the list as it stands for anything but a guide to the archeology of the collection upon which it reports. Its very painstaking compilation of distributional data can be utilized only after its taxonomy and nomenclature have been brought into line with modern concepts.

ALEXANDER B. KLOTS

FISHES OF WESTERN ARCTIC AMERICA AND EASTERN ARCTIC SIBERIA. *Taxonomy and Zoogeography. Bull. Amer. Mus. nat. Hist., Vol. 106, Art. 5.*

By Vladimir Walters. American Museum of Natural History, New York. $1.50 (paper). Pp. 255–368; ill. 1955.

Prepared as a doctoral dissertation, this treatise reviews what is known about the fishes of the Arctic seas and tributary fresh waters between longitudes 105°W. and 105°E., and it analyzes this fauna rather plausibly, though by the nature of the evidence not very conclusively, in terms of a variety of factors. These factors, involving geological and ecological changes and the physiological responses of the organisms, include climatic changes during Pleistocene and Recent time, temperature and salinity tolerances and preferences, dispersals, changes in the distribution of perennial ice and snow, and relative alterations in sea and land levels. In the reconstruction of past faunal changes leading to the present zoogeographical relations especial attention is given to a history, to a considerable degree hypothetical, of the Bering Strait Land Bridge, with particular reference to the barriers to the dispersal of some organisms, and the aids in the dispersal of others that have been presented by the land or sea connections, by shallow and brackish grading into deeper and saline straits and continental shelves, by formerly connected stream courses subsequently flooded to disrupt the freshwater faunas, etc. Postglacial reinvasions from Pleistocene preserves are hypothesized in some detail.

Praiseworthy is the author's rejection from the nomenclatural system of presumably phenotypic morphae and of the various infra-subspecific characters recognized by a few, mainly Soviet systematists; praiseworthy, also, is his defense of the subspecies category. Opinions will vary in reference to several taxonomic changes, chiefly in the direction of lumping, that the author has accepted or proposed. All will agree that he has added very materially to the knowledge and interpretation of one of the most interesting and extreme of the fish faunas. A few minor slips in fact and judgment we may expect to be rectified by other American students of the Arctic fishes (Wilimovsky, Dunbar, and others) and by the several Soviet ichthyologists who are investigating the same fauna. The current emphasis on Arctic ichthyology is very promising and gratifying.

CARL L. HUBBS

A SYSTEMATIC REVIEW OF THE MIDDLE AMERICAN FISHES OF THE GENUS *PROFUNDULUS. Misc. Publ. Mus. Zool. Univ. Mich., No. 92.*

By Robert Rush Miller. Museum of Zoology, University of Michigan, Ann Arbor. Free upon request (paper). ii + 64 pp. + 9 pl.; text ill. 1955.

Taxonomic, anatomical, geographical, ecological, variational, and speciational data are thoroughly developed and critically integrated in this exemplary revision, which was based on a prolonged study of nearly 10,000 specimens, in large part collected by the author. The species of *Profundulus* are confined to Middle America, which, as the author suggests, may represent the land mass in which the cyprinodont fishes originated. (I have long thought it probable that much of the differentiation of the group took place there, but where the order actually originated seems obscure.) The genus is authenticated and distinguished from *Fundulus* in large part on the basis of characters worked out by Miller—namely its broad and short premaxillary processes, the incompletely fused median hypural [a primitive char-

acter], and the numerous gillrakers. *Profundulus* is sharply divided into two groups, treated as subgenera. Two of the recognized species are localized forms, respectively interpreted as probably of Pliocene and Pleistocene origin. The other three are widespread, with, in each, much local differentiation that is so random and so inconsistent geographically as to have led the author to regard as unwise the recognition of subspecies. In certain meristic characters the trends from south to north are matched by clines from lower to higher elevations. The differentiation and dispersal of the group is reconstructed, with due regard to the geological as well as the morphological and zoogeographical data.

CARL L. HUBBS

THE REPTILE WORLD. *A Natural History of the Snakes, Lizards, Turtles, and Crocodilians.*

By Clifford H. Pope. Alfred A. Knopf, New York. $7.50. xxvii + 325 + xiv pp. + 129 pl. 1955.

Practically all herpetologists of the present generation were raised on Raymond L. Ditmar's books, especially *Reptiles of the World*. That volume, outstanding in its day, was a popularly and colorfully written summary and was widely treasured. Pope's *The Reptile World* is a worthy modern replacement. While Pope's presentation is less dramatic, it contains information that is more complete and accurate. It should have considerable appeal for high school students, general readers, and amateur naturalists.

There are four major sections: Crocodilians, Turtles, Snakes, and Lizards. Each of these sections begins with a general account, including topics such as size, age, growth, distribution, food, feeding, sense and intelligence, enemies and defense, reproduction, relations to man, popular beliefs, venom, hibernation, and locomotion. There follows a brief family-by-family review, with the better-known forms highlighted. The section is concluded with a short bibliography. A feature of the book is the inclusion of over 200 excellent photographs, mostly from living specimens.

The writing is clear and readable, although one gets the feeling that, in an attempt to use an appropriate popular style, Pope has on occasion been writing down to his audience. The effectiveness of the photographs would have been enhanced if indications of the sizes of the subjects were routinely given in the captions.

A spotty reader might criticize the author for his frequent anthropomorphisms, although Pope does point out, in his Introduction, that he uses them as short-cuts to avoid the circumlocutions that would be necessary in precise and accurate descriptions.

Pope's confident assertion that it is virtually impossible to exterminate sea turtles is at variance with the pessimistic view taken in Archie Carr's recent book, *The Windward Road*, in which the Green Turtle is compared with the American bison. Pope's statement that snakes do not leave a good fossil record will, fortunately, have to be altered in the light of new fruitful techniques that have permitted recent investigators to collect tremendous series of fossil snake vertebrae. The blurb on the jacket of the book states that readers will be able to use this book for identification purposes. Since *The Reptile World* contains neither keys nor detailed descriptions of species, it would seem that that statement requires some qualifications.

All in all, this is an excellent book. It is the best summary of the world's reptiles that is currently available.

ARNOLD B. GROBMAN

FINDING BIRDS IN MEXICO.

By Ernest Preston Edwards; illustrated by Frederick K. Hilton and the author. E. P. Edwards & Company, Amherst. $1.90 (paper). xx + 101 pp. + 7 pl.; text ill. 1955.

Six major faunal regions are recognized for Mexico. Each region is described by a map showing the principal towns and the paved roads; by a short general statement on the vegetation, climate, and commerce of the region; by a list of birds arranged according to the type of habitat in which they most frequently occur; and by a discussion of the best localities in the region to find birds, how to get there, and what to look for. Sample tours of the country are suggested. Camp-sites and other accommodations are listed. There is a list of Mexican birds (no local names); 7 plates depicting some of the more confusing species; a directory of paved portions of natural highways; and an index. Since all this information is compacted within 100 pages, it is obvious that not much space has been wasted. Nevertheless, the result is satisfactory. This book, together with some field guide, should enable any visitor to Mexico to derive the maximum benefit in bird-finding experiences.

HENRI C. SEIBERT

BIRD RECOGNITION. *III. Rails, Game-Birds and Larger Perching and Singing Birds.*

By James Fisher; illustrated by Fish-Hawk; maps compiled by W. B. Alexander; charts compiled by the writer. Penguin Books. 85 cents (paper). 159 pp.; ill. 1955.

Numbers one and two of this series have been previously noted and described (*Q.R.B.*, 30: 184. 1955). Number Three follows the original pattern and includes the game birds, rails, doves, swifts, kingfishers, woodpeckers, cuckoos, crows, starlings, orioles, larks, thrushes, and swallows.

HENRI C. SEIBERT

THE MONK SEALS (*Genus MONACHUS*). *Bull. Brit. Mus. (nat. Hist.) Zool., Vol. 3, No. 5.*

By Judith E. King. The British Museum (Natural History), London. 18s. (paper). Pp. 201–256 + 6 pl.; text ill. 1956.

ECONOMIC ZOOLOGY

INDEX IX TO THE LITERATURE OF AMERICAN ECONOMIC ENTOMOLOGY. *January 1, 1948–December 31, 1949. Spec. Pub. 9.*

Compiled by Ina L. Hawes. Entomological Society of America, Washington. $3.50. viii + 528 pp. 1954.

The publication of this two-year volume fills the gap left between Index VIII and Index X. The series is now complete through 1951. The geographic range of the present volume covers the United States, Alaska, Canada, Mexico, and the Panama Canal Zone. Some material published in Cuba, Puerto Rico, and Hawaii is also included.

The index is remarkably complete, readily understandable, and easy to use. There is no doubt that this volume will be of tremendous value to anyone even remotely connected with the field of entomology. The adjective "economic" in the title should be interpreted broadly, because the author has included many references which are not strictly economic but are judged to be of some interest to economic entomologists. For this liberal attitude and for an excellent job of compiling, the author is to be thanked.

V. G. DETHIER

INSECTICIDES AND COLONIAL AGRICULTURAL DEVELOPMENT. *Proceedings of the Sixth Symposium of the Colston Research Society held in the University of Bristol, March 23–27, 1953.*

Edited by T. Wallace and J. T. Martin. Butterworths Scientific Publications, London. 30s. 169 pp. + 1 pl.; text ill. 1954.

This book records a symposium of 16 papers on the problems of controlling insects in the colonies, primarily the African colonies, where peasant agriculture prevails. It is a practical treatise aimed at practical problems. Papers were given on the control of locusts, ticks, the tsetse fly, and other ectoparasites of animals. One paper dealt with storage and transit insects, two with cotton pests, one with design of field trials, one with biological control of pests, one with application techniques, and one with the manufacturers' problem.

JAMES G. HORSFALL

THE LIFE HISTORIES OF THE STEELHEAD RAINBOW TROUT (*SALMO GAIRDNERI GAIRDNERI*) AND SILVER SALMON (*ONCORHYNCHUS KISUTCH*) *with Special Reference to Waddell Creek, California, and Recommendations Regarding Their Management. Fish Bull., No. 98.*

By Leo Shapovalov and Alan C. Taft. State of California, Department of Fish and Game, San Pedro. Free upon request (paper). 375 pp.; ill. 1954.

Long awaited, and stemming from extensive field work begun in 1932, this report takes its place among a small series of intensive studies that describe in great detail a single representative trout or salmon stream and its inhabitants (the trout or salmon and their associates), with due regard to the complex interrelations in nature. The major portion of the results is integrated into accounts of the life histories of the steelhead and of the silver salmon, two of the most important fishes of the Pacific Coast. Migrations of the young and adult fish are particularly emphasized, on the basis of data gathered at the field station and experimental dam, where upstream as well as downstream traps were maintained. Spawning sites and spawning behavior, egg production, embryology and hatching, age and growth, seaward migration, sea life, homing and straying, survival, pathology, food at various ages, and predators are among the main subjects included in this encyclopedic investigation. The biological data are brought to bear on the practical problem of management, for which recommendations are made.

CARL L. HUBBS

AGE DETERMINATION OF THE NORTHERN ANCHOVY, *ENGRAULIS MORDAX. Fish Bull. No. 101.*

State of California, Department of Fish and Game, Marine Fisheries Branch, San Pedro. Free upon request (paper). 66 pp.; ill. 1955.

With the catastrophic decline of the sardine fishery, California fishermen, processors, and bait handlers turned their attention to previously little exploited species, particularly *Engraulis mordax*. This circumstance called for an understanding of the biology of this species. Investigators representing the several agencies participating in the California Cooperative Oceanic Fishery Investigations turned attention to the northern anchovy. In this report Daniel J. Miller of the state Department of Fish and Game presents extensive, detailed, and critical studies that validate for this fish the scale method for age determination, and, along with his colleagues Anita E. Daugherty of the same department and Frances E. Felin and John MacGregor of the Fish and Wildlife Service, following methods used in the sardine investigations, he reports, chiefly in tabular form and with a minimum of interpretation, their analyses of the age and length compo-

sition of this anchovy during the 1952–53 and 1953–54 seasons.

CARL L. HUBBS

FISHERY STATISTICS OF THE UNITED STATES 1952. *Statistical Digest 34.*

By A. W. Anderson and E. A. Power. United States Department of the Interior, Fish and Wildlife Service, Washington. $1.75 (paper). ii + 345 pp.; ill. 1955.

HUNTING WHITETAILS.

By Frank C. Edminister. William Morrow & Company, New York. $3.75. 192 pp. + 12 pl.; text ill. 1954.

The author of this book is a wildlife biologist by profession and a hunting enthusiast by avocation. With a background of 25 years' experience in both fields, the inevitable result is a book that will be read with interest and profit by sportsmen and biologists alike. The methods of deer hunting in the United States and the weapons used are discussed in detail. Archery hunting enthusiasts, growing from a handful before World War II to more than 100,000 at present, will be pleased to learn that the long bow and broadhead are not neglected. Instructions for dressing the deer, care of skin and trophy, cooking of venison, etc., are provided in detail for the enjoyment of the field hunter or his armchair counterpart. The final two chapters are concerned with whitetail ecology and psychology and offer an interesting account of deer hunting in North America from pre-colonial days to the present.

SCOTT S. PAULEY

BREEDING BEEF CATTLE FOR UNFAVORABLE ENVIRONMENTS. *A Symposium Presented at the King Ranch Centennial Conference.*

Edited by Albert O. Rhoad. University of Texas Press, Austin. $4.75. xiv + 248 pp. + 12 pl.; text ill. 1955.

This book is the published record of a symposium, held at the King Ranch in Texas, at which a series of 19 papers was presented by outstanding geneticists, physiologists, nutritionists, and animal production specialists from the United States, South America, Africa, and the British Empire. The symposium dealt with the general topic of the effects of climatic stress on the growth, reproduction, and selection of various types of cattle. Approximately half of the symposium was devoted to the effects of unfavorable environmental conditions upon the degeneration of British beef breeds when produced in a tropical environment, upon fertility, upon resistance to disease and parasitism, and upon the measurement of heat tolerance. The second portion of the book contains 4 chapters dealing with the application of genetic principles to cattle breeding, and 3 chapters describing the development of the Afrikander cattle in South Africa, the utilization of Zebu cattle (*Bos indicus*) in the improvement of Brazilian cattle, and the development of the Santa Gertrudis breed at the King Ranch. The remaining papers deal with topics such as how to meet nutritional requirements under unfavorable environmental conditions, and the probable role of research in future livestock improvement.

FREDERICK N. ANDREWS

FERTILITY AND INFERTILITY IN THE DOMESTIC ANIMALS, *Etiology, Diagnosis and Treatment. A Textbook for Veterinary Surgeons and Students of Agricultural and Veterinary Science.*

By J. A. Laing; foreword by John Hammond. Baillière, Tindall & Cox, London; [The Williams & Wilkins Company, Baltimore]. $7.50. xii + 256 pp. + 28 pl.; text ill. 1955.

This text is written for students of veterinary medicine, practicing veterinarians, and university students of reproductive physiology. The first 5 chapters deal with the physiology, endocrinology, and histology of the estrous cycle, ovarian and testicular activity, and semen characteristics of cattle, sheep, swine, the horse, and to a lesser extent the dog. Separate chapters are devoted to brucellosis, trichomoniasis and genital infection with *Vibrio foetus*, and tuberculosis; 4 additional chapters describe the more common infectious and non-infectious diseases of the male and female genitalia.

The material is well documented with references to British and American workers, especially prior to 1950, but is somewhat deficient with respect to current researches in the physiology of the ovum and sperm. Unfortunately, most of the literature cited was published prior to 1950. The book is interestingly written and well illustrated, and should prove of real value to veterinary and agricultural students in British and American universities.

FREDERICK N. ANDREWS

CATTLE FERTILITY AND STERILITY.

By S. A. Asdell. Little, Brown & Company, Boston and Toronto. $5.50. x + 227 pp.; ill. 1955.

This text, devoted exclusively to cattle, should prove valuable to upper-class university students in dairy and animal husbandry, to veterinarians, and to pro-

fessional cattlemen. It summarizes the generally accepted facts of the reproductive physiology of the bovine, and gives a clear account of the anatomy and histology of the genitalia, the estrous cycle, gametogenesis, and semen quality, and the principal causes of infertility. It is written in a concise, clear style, with a minimum number of references.

FREDERICK N. ANDREWS

CATTLE & MEN.

By Charles Wayland Towne and Edward Norris Wentworth. University of Oklahoma Press, Norman. $4.00. xiv + 384 pp. + 24 pl.; text ill. 1955.

In an earlier volume, the two authors combined their talents for historical story-telling to produce a delightful biography of the pig "from cave to corn belt." In it they showed a fine sense of balance, juggling statistical fact, historical significance, and humorous incident with telling effect. They have now turned their attention to cattle, and the results are just as readable and as elucidating. Their topic is not all-inclusive, however, for they restrict themselves to a discussion of beef cattle, the dairy breeds having been omitted.

The authors take their cattle through the evolutionary stages from the ancient aurochs and its relatives to the highly selected shorthorn and Brahmin of today, trace their development which began in Europe and Asia and which was eventually transferred to our wide western plains, and describe and discuss their influence and role in the economy of the developing West. The result is an able history of an important segment of American agriculture.

C. P. SWANSON

DAIRY CATTLE, *Selection, Feeding, and Management. Fourth Edition.*

By William Wodin Yapp and William Barbour Nevens. John Wiley & Sons, New York; Chapman & Hall, London. $4.76. xii + 420 pp. + 2 pl.; text ill. 1955.

This is the fourth edition of a textbook dealing with the elementary principles of dairy cattle and milk production, written primarily for the student of vocational agriculture, the 4-H club member, and the practical dairyman. It presupposes no formal training of its readers in the biological sciences; and the chapters dealing with the inheritance of milk production, breeding management, milk secretion, and feeding are in simple, non-technical language. The text is sufficiently up to date for its intended readers, but not sufficiently current for the university student or for the specialist in breeding or nutrition.

FREDERICK N. ANDREWS

ANIMAL GROWTH AND DEVELOPMENT

A MANUAL OF ORAL EMBRYOLOGY AND MICROSCOPIC ANATOMY.

By Dorothy Permar. Lea & Febiger, Philadelphia. $3.75. 109 pp.; ill. 1955.

This small textbook is designed for dental hygiene students. It is a simple, straightforward presentation of dental histology and embryology at a very elementary level. The illustrations are simple to the point of crudeness. It should be satisfactory for its stated purpose.

F. N. LOW

ANIMAL MORPHOLOGY

TEXTBOOK OF ANATOMY AND PHYSIOLOGY. *Thirteenth Edition.*

By Diana Clifford Kimber and Carolyn E. Gray; revised by Caroline E. Stackpole and Lulie C. Leavell. The MacMillan Company, New York. $5.50. xii + 850 pp.; ill. 1955.

This textbook (12th ed., *Q. R. B.*, 24: 156. 1949) is designed primarily for use in nursing education and is therefore organized and presented at an elementary level. Its clarity and readability are especially commendable. The 388 illustrations are mostly line drawings, but a goodly number of them have diagrammatic colors. This carefully planned book should be useful not only for its intended group, but for students with some foreknowledge of biology at the junior college level.

F. N. LOW

L'ANATOMIE EN POCHE. *6th Edition.*

By Victor Pauchet and S. Dupret. G. Doin & Cie., Paris. 1,400 fr. (paper). 464 pp.; ill. 1953.

This little pocket anatomy is intended for review by those who need an easy source of anatomical reference. There is no text, the entire book consisting of labeled anatomical drawings with alphabetical and analytical indices at the back. The illustrations are mostly line drawings in black and white, but some contain different shades of red. The drawings are clear and the book is accurate enough, but its usefulness is limited to the purposes stated above.

F. N. LOW

LEHRBUCH UND ATLAS DER ANATOMIE DES MENSCHEN. *19th Edition. Band II. Eingeweide, Nervensystem—Sinnesorgane.*

By Fr. Kopsch. Georg Thieme Verlag, Stuttgart. DM 16.50; $15.35. viii + 768 pp.; ill. 1955.

The first volume of this distinguished German anatomy text was reviewed recently (*Q. R. B.*, 30: 407. 1955) with due praise for its authoritative quality. It should be noted, however, that, while the descriptive anatomy is accurate and quite acceptable, little attempt has been made to include recent work of current interest. As a case in point, there is no account of the bronchopulmonary segments, a field in gross anatomy that has received considerable attention in the past few years and has contributed much of interest to thoracic surgery. Also, there is a very strong tendency to limit references to European investigations, sometimes to the extent of completely ignoring important British and American contributions. Nevertheless, this is a good anatomy text that will serve its readers well. It should be especially useful to American investigators as a source book for recent European contributions.

F. N. Low

SEGMENTAL ANATOMY OF THE LUNGS. *A Study of the Patterns of the Segmental Bronchi and Related Pulmonary Vessels.*

By Edward A. Boyden. The Blakiston Division, McGraw-Hill Book Company, New York, Toronto, and London. $15.00. xx + 276 pp.; ill. 1955.

Boyden's exhaustive and scholarly study of lung anatomy is bound to be recognized as the definitive work in its field. It may be somewhat surprising that the gross anatomy of the lung had been worked out so haphazardly that, when surgical removal of only a portion of a lung became feasible, it was necessary to dissect anew to achieve a satisfactory formulation of the bronchopulmonary segments and their associated blood vascular trees. But Boyden found it necessary to do just this, an undertaking that required 9 years of intensive personal investigation and collaboration. The exceedingly complex and detailed findings arising from this study have been drawn together and presented under one cover in this distinguished contribution.

Each of the 5 lobes of the lungs is systematically described and illustrated under headings of surface anatomy, bronchial distribution, and arterial and venous patterns. The data for each lobe are drawn from either 50 or 100 dissections of the area. Significant variations are described. An historical review opens the book; and there are chapters on the hilar portions of the interlobar fissures, lung embryology, and the lung as a whole. The illustrations are mostly clear line drawings, many with color, but there are roentgenograms and half-tones. There are a bibliography and an index.

Boyden undertook this exacting and difficult task as he was approaching retirement, when most careers would be drawing to an inactive close. Yet the energy, perseverance, and painstaking accuracy evident throughout the pages of this book would put to shame many a more youthful investigator. It is difficult to find flaws in this notable monograph. It should be in the library of everyone interested in the lung.

F. N. Low

ANIMAL PHYSIOLOGY

THE PHYSIOLOGY OF INSECT METAMORPHOSIS. *Cambridge Monographs in Experimental Biology, No. 1.*

By V. G. Wigglesworth. Cambridge University Press, New York. $2.50. viii + 152 pp. + 4 pl.; text ill. 1954.

Aside from the central theme of evolution, biology suffers from the lack of unifying principles. Consequently, one of the aims of this book, to forge a link between insect metamorphosis and polymorphism, is to be appreciated.

Wigglesworth proposes to demonstrate "that metamorphosis is merely a special case of a general phenomenon, which is polymorphism in the widest sense." A hierarchy of processes which result in "polymorphism" at the various levels of biological organization is presented in support of this idea. At the levels of cells, tissues, organs, and organism the processes of determination and differentiation are at work, involving interaction of genome, cellular substrate, inductors, and hormones. At higher levels of polymorphism, concerned with differences between individuals of the same species, form-determining elements may arise externally to the individual to supplement those active at lower levels. These factors might be variation in the food supply or such things as termite social hormones. Hence, in the words of the author, "Determination at all levels of complexity is thus controlled by the supply or deficiency of specific raw materials." What does this really tell us about the relationship between insect development and polymorphism?

What has actually been said seems to be that both metamorphosis and polymorphism are like all other physiological processes in that their biochemical systems are not independent of the environment. Viewed in this light, what has been demonstrated is that metamorphosis and polymorphism are merely special cases of a general phenomenon, which is metabolism in the widest sense!

JAMES F. CASE

HUMAN PHYSIOLOGY. *Second Edition.*

By Bernardo A. Houssay, Juan T. Lewis, Oscar Orias, Eduardo Braun-Menéndez, Enrique Hug, Virgilio G. Foglia, and Luis F. Leloir; translated by Juan T. Lewis and Olive T. Lewis; foreword by Herbert M. Evans. McGraw-Hill Book Company, New York,

Toronto, and London. $12.00. xvi + 1177 pp.; ill. 1955.

THE PHYSIOLOGICAL BASIS OF MEDICAL PRACTICE. *A Text in Applied Physiology. Sixth Edition.*

By Charles Herbert Best and Norman Burke Taylor. The Williams & Wilkins Company, Baltimore. $12.00. xiv + 1357 pp. + 4 pl.; text ill. 1955.

A TEXTBOOK OF PHYSIOLOGY. *Seventeenth Edition.*

Edited by John F. Fulton; with the collaboration of Donald H. Barron, William D. Blake, John R. Brobeck, George R. Cowgill, Paul F. Fenton, Thomas R. Forbes, Samuel Gelfan, David I. Hitchcock, Hebbel E. Hoff, David P. C. Lloyd, Theodore C. Ruch, and Jane A. Russell. W. B. Saunders Company, Philadelphia and London. $13.50. xlii + 1275 pp.; ill. 1955.

These three of the six great textbooks of human physiology are now issued in new editions with varying degrees of revision, the one by Best and Taylor having undergone the most extensive rewriting. All are well-balanced accounts despite the fact of slight differences in emphasis which depend on the editor's or authors' own work. Therefore, as one might expect, the sections on digestion, nutrition, and metabolism are more complete in Best and Taylor's; greater attention is paid to the endocrines in Houssay's; and a slightly more extensive (and better organized) treatment of the nervous system and special senses is found in Fulton's. However, all three are quite adequate in the handling of this material. Also, as one would anticipate by the number of contributors to each book, the unity is best in the "Best and Taylor." The English version of Houssay's textbook does not suffer in translation from the Spanish, as it was made by one of the authors (Juan Lewis). Indexing is superior in Best and Taylor and in Houssay, but is somewhat wanting in Fulton's textbook (although the Table of Contents in the latter is given more detailed consideration). On the other hand, the arrangement of references (at the end of each section, a form which I myself prefer) is superior in Fulton's. Any one of these textbooks is suitable for a medical school course in physiology.

DAVID B. TYLER

RECENT PROGRESS IN HORMONE RESEARCH. *The Proceedings of the Laurentian Hormone Conference. Volume X.*

Edited by Gregory Pincus; committee on arrangements, R. W. Bates, G. Grant, R. D. H. Heard, A. D. Odell, E. C. Reifenstein, Jr., A. White, and G. Pincus. Academic Press, New York. $9.80. vi + 511 pp.; ill. 1954.

The papers included in this volume are grouped into five sections, as follows: (I) The Central Nervous System and Stress-induced Eosinopenia (R. W. Porter); On Brain Metabolism and Electrical Activity in Relation to Adreno-cortical Physiology (H. Hoagland); On Hormonal Effects on Brain Excitability and Electrolytes (D. M. Woodbury). (II) Triiodothyronine and Thyroid Physiology (J. Gross & R. Pitt-Rivers); On Metabolic Effects of Thyroid Hormones in Vitro (H. A. Lardy & G. F. Maley). (III) Endocrine Mechanisms in Insect Life (D. Bodenstein); Hormones Prepared by Neurosecretory Cells (E. & B. Scharrer). (IV) The Preparation and Chemistry of Crystalline Insulin (R. G. Romans); The Chemistry of the Corticotrophins (E. E. Hays & W. F. White). (V) Endocrine Factors and Radiation-induced Lymphoid Tumors in Mice (H. S. Kaplan, C. S. Nagareda, & M. B. Brown); Endocrine Influences upon Formed Elements of Blood and Blood-forming Organs (A. S. Gordon). (VI) Problems of Ovarian Function and of Pregnancy (B. Zondek); Clinical Studies on Electrolyte and Fluid Metabolism (R. Luft, B. Sjögren, D. Ikkos, H. Ljunggren, & H. Tarukoski); and A Comparison of Steroidal Excretion and Metabolic Effects Induced in Man by Stress and by ACTH (J. W. Conn, S. S. Fajans, L. H. Louis, H. S. Seltzer, & H. D. Kaine).

To review such a work in detail is virtually impossible, but it should be pointed out that the high standard set in the previous numbers of this series is fully maintained both in form and in content. It is also worthy of note that for the first time considerable space is devoted to invertebrate hormones. Reviewers of other symposia have recently challenged the inclusion of discussions. No doubt there are discussions that are well forgotten. These, however, are based on first-hand knowledge in the field, are generally pertinent, and add substantially to the value of the book.

GAIRDNER B. MOMENT

MAN IN A COLD ENVIRONMENT. *Physiological and Pathological Effects of Exposure to Low Temperatures. Monogr. Physiol. Soc., No. 2.*

By Alan C. Burton and Otto G. Edholm. Edward Arnold, London; [Williams & Wilkins Company, Baltimore]. $6.75. xiv + 273 pp.; ill. 1955.

Within two years of its publication, the compilation edited by Newburgh, and entitled *Physiology of Heat Regulation* (*Q. R. B.*, 26: 219. 1951), was virtually out of print, almost before a reviewer could predict that it was destined to be a "classic" in the field. The present publication was written partly to meet the need for a current textbook in thermal physiology, and partly to add the experience and findings of British and Canadian workers to those of United States workers, so ably presented in the former volume. What it loses by its less ambitious scope, it gains by simplicity of style and direct presentation. As might be expected from the nature of the subject and the predilections of the authors, the first half of the book deals mainly with the physical principles of heat transfer between man and a

cold environment, with considerable emphasis upon the use of Burton's simplified equations for the expression of that transfer. The latter half describes the physiological responses of the human body to cold, with limited discussion of the clinical physiology of local and general hypothermia.

Unlike its predecessor, this volume can be read intelligently by those who have had little formal training in physics or physiology, and it loses little in the process. The discussion becomes a trifle erudite here and there, and unfortunately the opening chapter errs most in this respect. Prospective readers would be well advised to omit this chapter and come back to it in a critical spirit later, with a wider knowledge of the subject gleaned from later chapters. Fig. 36 gave—and still gives—the reviewer a lot of trouble in trying to reconcile the units of the abscissa with the legend, and with text examples. The emphasis placed upon increased caloric requirements in the cold may turn out to be too great in the light of recent investigations. The special differential use of the terms "acclimatization," "acclimation," and "adaptation," as suggested at the beginning of Chap. 10, is difficult to justify either in practice or in philology.

The weaknesses mentioned, however, are no more than might be expected from specialists writing enthusiastically in their own field, and will be recognized as such by the alert reader. The book as a whole can be confidently recommended as a readable, yet authoritative, text for those who desire a background rationale for dealing with problems of cold environments.

DOUGLAS H. K. LEE

RECENT PROGRESS IN HORMONE RESEARCH. *Proceedings of the 1954 Laurentian Hormone Conference. Volume XI.*

Edited by Gregory Pincus. Academic Press, New York. $10.00. viii + 518 pp.; ill. 1955.

This volume contains the lectures (and highlights of the discussion following them) given at the 1954 Laurentian Hormone Conference. Van Dyke, Sonenberg, Munson, Hertz, and their collaborators deal in separate papers with the pituitary hormones; Fried and his collaborators, Simpson, and Tait discuss the chemistry and biochemistry of steroid hormones; Furth, Rawson and Rall write about experimental tumors and the endocrinology of neoplastic disease, respectively; Engle, Pincus, Dorfman, and their collaborators are concerned with hormones and the aging process; Levine's article on the translocation mechanism of insulin action is especially good, and Hastings gives an elegant summary of the present state of affairs on the effects of ions and hormones on carbohydrate metabolism; Katz and others consider in some detail dietary and hormonal factors in experimental atherogenesis and blood pressure regulation. Shorr et al. discuss the chemical and biological properties of VEM and VDM, the so-called hepatorenal factors. One might add that the papers, as they appeared to be presented, showed that the various authors exercised, or at least tried to exercise, some restraint. In fact, all were on their good behavior until the discussion started. Then they behaved as one might expect endocrinologists to behave, who have differing points of view.

DAVID B. TYLER

THE HORMONES. *Physiology, Chemistry and Applications. Volume III.*

Edited by Gregory Pincus and Kenneth V. Thimann. Academic Press, New York. $22.00. xiv + 1012 pp.; ill. 1955.

Thimann in an excellent review of plant growth hormones here develops the concept that the auxin system exerts an influence on the over-all hormonal control of plant growth and development. Scharrer considers hormones in invertebrates, particularly in worms, crustaceans, anthropods, mollusks, and tunicates, and shows that there is a remarkable parallelism in the basic mechanisms of hormone action between invertebrates and vertebrates. The review on the neurohormones (acetylcholine, adrenaline, nor-adrenaline, dopamine, serotonin, oxytocin, and vasopressin) by Welsh is excellent. He develops the theme that a characteristic feature of neurons is their production and release of substances required for the integration of bodily functions. Greep and Kenny review briefly the recent literature dealing with the physiological activity of the parathyroid, particularly the phosphaturic and calcium-mobilizing actions. Stetten and Bloom present the recent findings on the chemistry, physiology, and mechanism of action of insulin and glucagon. Recent developments in standardization and isolation of the anterior pituitary hormones, as well as the newer information on their physicochemical properties, are described by Hays and Steelman; and in a separate and thorough review Astwood deals with the physiological functions of the growth hormone and corticotropin. The comprehensive account of the physiology of the gonadotropins and of the lactogenic hormone, together with the reviews of these hormones in the previous two volumes of this series, now give a complete and up-to-date picture of the state of development of these subjects. In the paper on the hormones of the posterior pituitary, Landgrebe, Ketterer, and Waring are principally concerned with the chemistry and physiology of the neural lobe substances and touch briefly on the melanophore-excitant substance from intermediate lobe extracts. A comprehensive treatment by Rawson, Rall, and Sonenberg brings the literature on the thyroid up to date, particularly by considering those studies reported since 1949. In two separate and comprehensive reviews Hirschmann and Dorfman bring up to date the

present information on the chemistry and metabolism, respectively, of the steroid hormones. Pincus, in a concise and short review, presents a synopsis of the physiology of the ovarian and testicular hormones. Noble, in one hundred pages of text, summarizes the findings reported in over 1260 papers, during the past 5 years, on the physiology of the adrenal cortex. These of course are not all of the papers published during that interval on the function of the adrenal cortex; but it gives one a rather good idea of the state of saturation of the medical literature on the subject. Paschkis and Rakoff present the recent advances in principles of and approaches to the clinical aspects of endocrinology which should be of interest to the practicing physician or student.

DAVID B. TYLER

ANIMAL NUTRITION

STANDARD VALUES IN NUTRITION AND METABOLISM, *being the second fascicle of a Handbook of Biological Data.*

Edited by Erreti C. Albritton; prepared under the direction of the Committee on the Handbook of Biological Data, American Institute of Biological Sciences, The National Research Council. W. B. Saunders Company, Philadelphia and London. $6.50 (paper). xiv + 380 pp.; ill. 1954.

This second fascicle of the *Handbook of Biological Data* contains tables of data that have the greatest likelihood of reproducibility. The contents deal specifically with the nutrients used by plants and animals, their daily allowances, energy values, energy exchange, and pathways and end-products of metabolism. Also included here are data on the O_2 consumption and CO_2 production of various animal and plant tissues and organisms.

DAVID B. TYLER

BIOCHEMISTRY

BIOCHEMISTRY. *An Introductory Textbook.*

By Felix Haurowitz. John Wiley & Sons, New York; Chapman & Hall, London. $6.75. xvi + 485 pp.; ill. 1955.

During the past few years there have appeared several good textbooks of biochemistry. Some of these books have been intended for medical students, some for agricultural students, others for students of biology or home economics. Biochemistry, however, is a branch of science in its own right and needs no specialized interpretation, at least at the introductory level. This book by Haurowitz recognizes that fact and presents a unified picture of the field of biochemistry. It is intended for Seniors and graduate students and presumes an adequate background in chemistry. No time is lost in backtracking through general chemistry, analytical chemistry, or organic chemistry.

Since the primary aim of biochemistry is to understand the mechanism of metabolic changes in the living organism, this material is covered very early in the book. There follow chapters on carbohydrates, proteins, lipids, nucleic acids, porphyrins, minerals, and enzymes. Not only the chemistry of these compounds is discussed, but their metabolism as well. There is a special chapter on energy balances and nutrition and another on human biochemistry. Each chapter has a list of references to books and review articles.

The book is well organized and interestingly written. It should find a wide acceptance, not only as a textbook, but also as a reference work for those who desire a balanced presentation of modern biochemistry.

G. R. NOGGLE

LABORATORY EXPERIMENTS IN BIOCHEMISTRY.

By Edwin Theodore Mertz and John Willard Porter. Burgess Publishing Company, Minneapolis. $2.50 (paper). viii + 67 pp.; ill. 1955.

The authors indicate that this manual is intended for students of agriculture, home economics, biology, and chemistry who want a broad, general background in elementary biochemistry. Biochemistry deserves a better fate. Most of the experiments are elementary exercises in chemical physiology. On such a diet it would be surprising if any student were to generate enough interest in biochemistry to want to go on in the field.

About a third of the book is devoted to miscellaneous items in general chemistry and analytical chemistry. The remaining experiments deal with proteins, lipids, and carbohydrates. No basic principles of biochemistry are discussed, nor are any of the newer techniques demonstrated.

G. R. NOGGLE

ENZYMOLOGIE. *Eine Darstellung für Chemiker, Biologen und Mediziner.*

By Otto Hoffmann-Ostenhof. Springer Verlag, Wien. $26.65. xvi + 772 pp.; ill. 1954.

I approached this book with some doubt as to whether the work of a single author could compete with the well-known treatises on enzymes in which each chapter is written by a specialist in his respective field. The answer is in the positive. Hoffmann-Ostenhof has succeeded in collecting in this book an enormous amount of material and in presenting it systematically and in lucid language. Since there does not exist a logical classification of enzymes, the author had to use a new type of classification and had to create new systematic

names. It is this systematic approach and the uniform outline of the book which make it so valuable. Orientation here is much easier than in a treatise written by numerous authors. The principal part of the book, in which the different enzymes are discussed systematically, is preceded by short chapters in which the history of enzyme research, thermodynamics, and other problems of general enzymology are discussed. In the last chapters of the book the role of enzymes in metabolic reactions and the multi-enzyme systems are discussed. The book has an extensive subject index and an author index with approximately 3000 names. The book is highly recommended as a very valuable source of material in the field of enzymology.

FELIX HAUROWITZ

CHEMICAL SPECIFICITY IN BIOLOGICAL INTERACTIONS. *Edited by Frank R. N. Gurd. Academic Press, New York.* $6.00. xvi + 234 pp.; ill. 1954.

The present volume represents the third of a series of symposia held at Harvard University upon some phase of protein science. Its emphasis is on a number of particular aspects of the chemical specificity underlying biological interactions. A good deal of attention is given to metal-protein interactions in a series of chapters by several specialists who are working in related areas. Other problems of chemical specificity in the fields of nucleic acids, radiation injury, steroid hormones, and peptide hormones are discussed.

The first chapter, which bears the same title as the book, was written by Edwin J. Cohn and was not yet complete at the time of his death (October 1, 1953). It was subsequently completed by the editor and deals for the most part with the interactions of proteins with metals and the properties of such systems. The last four chapters are also concerned with various aspects of metal-protein complexes. C. D. Coryell, in Special Problems in the Formation of Metal Complexes, discusses some of the problems in metal complex formation such as the ionic-covalent transition in iron-group complexes and the specificity in complex formation of hemoglobin and ferrihemoglobin. The chapters Interactions of Metals with Small Molecules in Relations to Metal-Protein Complexes, by Jack Schubert, The Interaction of Proteins with Small Molecules and Ions, by G. Scathchard, W. L. Hughes, Jr., F. R. N. Gurd, and P. E. Wilcox, and The Specificity of Metal Complex Formation, by G. Schwarzenbach, round out the subject of complexes and the interaction of small molecules and ions with proteins. R. B. Turner, in Physical and Chemical Properties of the Steroids Related to Protein Binding, discusses the various properties of the steroids with reference to the possible modes of binding in steroid-protein interactions. T. F. Gallagher writes a brief obituary of the late Konard Dobriner before presenting a chapter on Biochemical Problems of the Steroid Hormones. The latter is a treatment of our present state of knowledge of the metabolism of these hormones and the unsolved problem of their mechanism of action in the control and regulation of chemical processes. The chapter on Effects of Radiation on Proteins and Living Tissues, by S. Warren, represents a basic and interesting analysis of the phenomena involved. Waldo E. Cohn's paper, The Influence of Ion-Exchange Chromatography upon our Concept of the Structure of Ribonucleic Acid, reviews the evidence for ribonucleic acid structure in an excellent presentation. The chapter by V. Du Vigneaud, Some Studies on the Active Principles of the Posterior Pituitary Gland, covers for the most part the brilliant work of the Cornell group on the purification and characterization of the hormones oxytocin and vasopressin. The final studies on structure and synthesis are not included, since they were performed subsequently.

This volume is recommended as a valuable and interesting addition to the bookshelf of either the biochemist or the physiologist. Its timeliness and fundamental subject matter, presented in such compact fashion, make it highly desirable.

ALVIN NASON

THE BIOCHEMISTRY OF VITAMIN B_{12}. *A Symposium held at The London School of Hygiene and Tropical Medicine on February 19, 1955. Biochem. Soc. Symposia, No. 13.*

Organized and edited by R. T. Williams. Cambridge University Press, New York. $3.75. vi + 123 pp.; ill. 1955.

This book was written by a group of European scientists who have been actively engaged in the investigation of various facets of vitamin B_{12}. Its publication is most timely, since the interest in this versatile nutrient has been broadened as a result of extensive research on the metabolic functions of the vitamin and its physiological role in health and in diseases. This book covers adequately the chemistry and isolation of vitamin B_{12}. The chapter on chemistry is well written, by an author who himself has made some most important contributions to knowledge of the chemical structure of vitamin B_{12}. Other chapters include a discussion of factors related to vitamin B_{12} as well as the methods of measuring it.

Although vitamin B_{12}, as a member of the B complex, is very soluble in water, the mechanisms of absorption following oral administration and of excretion following parenteral administration are entirely distinct from those of other water-soluble vitamins. Besides, the absorption of the orally administered B_{12}, unlike other vitamins, requires the presence of Castle's intrinsic factor. This problem as to absorption is comprehensively discussed in a chapter by Mollin and Baker,

leading investigators in this field. Although the chemical nature of the intrinsic factor is not clearly understood, a useful chapter on this subject has been prepared with an extensive bibliography, which is incomplete only because a number of papers of paramount importance appeared in the literature after the book was written.

Two interesting chapters on the metabolic function of this vitamin are included. Although they are well written, it should be borne in mind that the vitamin's functions are still far from being fully understood. This book should be very useful to biochemists in general and to nutritionists in particular.

BACON F. CHOW

BIOCHEMISTRY AND PHYSIOLOGY OF PROTOZOA. *Volume II.*

Edited by S. H. Hutner and André Lwoff. Academic Press, New York. $9.00. xiii + 388 pp.; ill. 1955.

The second volume of this work is a welcome addition to the first (*Q. R. B.*, 28: 202. 1953). Like that volume, it is a series of reviews without any attempt at coordination between them. This has led to some overlap between the chapters in the two volumes and to a great diversity of approach. However, this fact merely reflects the present state of protozoan physiology and biochemistry. Some parts are relatively well explored; only fragmentary information is available about others. The content of the book is best shown by listing the chapter titles and authors, which are as follows: Introduction, by S. H. Hutner; Comparative Biochemistry of Flagellates, by S. H. Hutner and Luigi Provasoli; Composition and Synthesis of the Starch of *Polytomella coeca*, by S. A. Barker; The Nutrition of Ciliates, and Encystment and Excystment of Protozoa, both by W. J. van Wagtendonk; Metabolism of Free-Living Ciliates, by Gerald R. Seaman; Mutualistic Intestinal Protozoa, by R. E. Hungate; Developmental Physiology of the Ameboid Slime Molds, by Maurice Sussman; The Chemotherapy of Malaria, Piroplasmosis, Trypanosomiasis, and Leishmaniasis, by L. G. Goodwin and I. M. Rollo; and Comparative Studies on Amebae and Amebicides, by William Balamuth and Paul E. Thompson.

As is pointed out in the Introduction, the protozoa offer a wealth of opportunities for the comparative physiologist and biochemist. These volumes will help stimulate and guide the intelligent use of these opportunities. Those of us who are using the protozoa for other purposes must be grateful to the editors and contributors for bringing together and critically evaluating the widely scattered literature.

R. F. KIMBALL

THE ELEMENTARY CHEMICAL COMPOSITION OF MARINE ORGANISMS. *Memoir Sears Foundation for Marine Research, Number II.*

By A. P. Vinogradov. Sears Foundation for Marine Research, Yale University, New Haven. xiv + 648 pp.; ill. 1953.

It is difficult to imagine the time and effort that must have been expended by Vinogradov and his coworkers in bringing together the material that is in this volume. The tabulation by itself would have been a major contribution to the literature of biogeochemistry. The 327 numbered tables, listing organisms, composition, locality of collection, and reference to the original literature, supported by a bibliography of over 2,250 entries, give an indication of the bulk of factual material contained in the volume. This is presented in 18 chapters, each devoted to a class or order of marine organisms. Within each chapter the arrangement generally is to proceed from bulk composition—water, organic content and ash, through the individual major constituents—carbon, nitrogen, phosphorus, etc., to the minor constituents.

To call attention to the tabulated material singles out a major part of the bulk of the volume. However, to do so neglects that part which carries the book from being merely an excellent set of tables to a major contribution to the literature of biogeochemistry. Throughout the entire volume Vinogradov's search for unifying concepts is evident from his continual synthesis and rearrangement of groups of data, and from his attempts first to evaluate the role of marine organisms in the cycle of matter through the biota and the inanimate state, and secondly to estimate the extent to which the observed distribution of elements in nature has been influenced by organisms in the seas. It is here, especially in the first and last two chapters, respectively Introduction, The Regulating Influence of Ocean Salts on The Chemical Composition of Marine Organisms, and Fundamental Changes in The Elementary Composition of Marine Organisms during Geological Time, that Vinogradov the scientist and astute interpreter of his and other's works, is evident.

Only 1500 copies of this edition were printed. This is perhaps an indication of the relative scarcity of workers in biogeochemistry and closely related fields. It seems likely that, because of the continuing increase of interest in the earth sciences, the obvious potential for expansion in the biogeochemical fields, and the excellence given this volume by Vinogradov and the American editorial staff that produced it, this book may in time become a collector's item.

DAYTON E. CARRITT

STUDIES OF BIOSYNTHESIS IN *ESCHERICHIA COLI*. *Carnegie Inst. Wash., Pub. 607.*

By Richard B. Roberts, Philip H. Abelson, Dean B. Cowie, Ellis T. Bolton, and Roy J. Britten. Carnegie Institution of Washington, Washington. $3.00 (cloth); $2.50 (paper). xiv + 521 pp.; ill. 1955.

This book is a report of the authors' research and brings together various papers scattered through the literature, and some unpublished work. The significance of the results taken as a whole is discussed in the text. Its early chapters contain details of experimental techniques, including the incorporation of isotopes and characteristic patterns of labeling of metabolites. These chapters can be very valuable for any investigator of *E. coli* or related organisms. The later chapters interpret the assembled data in terms of biosynthetic mechanisms.

Textual citations have been kept to a minimum, but a general bibliography of about 1300 references on the biochemistry of *E. coli* appears in the book. This bibliography covers the period January 1940 to May 1954. The references are arranged only by subject; the bibliography would be more useful with an index of authors. Without pretending to cover all the aspects of *E. coli* biosynthesis, or to exhaust any particular subject, the book gives a balanced picture of the present knowledge of "*E. coli* biochemistry." This publication is recommended as a useful reference for any microbiological research laboratory.

BORIS ROTMAN

BIOCHEMICAL INVESTIGATIONS IN DIAGNOSIS AND TREATMENT.

By John D. N. Nabarro. Little, Brown & Company, Boston. $6.00. x + 299 pp. 1955.

The growing complexity of biochemical techniques and their application to clinical problems has produced a pressing need for a book to interpret the newer methods in terms of clinical problems. This is the avowed intention of the author, but this book falls far short of the modest goals set in the Preface. It is written with little or no attempt to interpret in clinical terms the biochemical concepts which have been accepted for decades. The author decries the works of biochemists who try to interpret their science to the clinician—on the legitimate ground that they have insufficient clinical knowledge to understand the clinician's viewpoint. It is clear, however, that the author is not sufficiently conversant with the rudiments of inorganic and organic chemistry to accomplish his task. For example, in discussing the bicarbonate buffer system, the following erroneous statement is made:

"It [carbon dioxide] is dissolved in these fluids as $H \cdot HCO_3$." Only a minute fraction of the dissolved CO_2 of the body fluids is actually present as acid.

In the discussion of the CO_2-combining power of the plasma, there is a complete lack of understanding of the parameters represented by the values reported by the laboratory.

Deficiencies in interpretation and clarity abound in all sections of this volume. The best discussions deal with calcium metabolism. Porphyrin excretion is dealt with in two places without any mention of the highly important fact that therapeutic doses of barbiturates can evoke marked coproporphyrinuria. Furthermore, by implying that coproporphyrin determinations cannot be done without an ultraviolet spectrophotometer, the author ignores the simple clinical tests which are available for this estimation.

The discussion of salicylate intoxication suggests that toxic blood levels of salicylate are around 100 mg%, and that 35 mg% is a therapeutic level. In my own experience, most severe and even fatal cases in children occur with blood levels in the thirties or below.

This must be set down as a hastily written, badly organized, sketchy catalogue of unconnected laboratory data, accompanied by facile, ad hoc explanations which are, in many cases, incredible. As a clinical approach the book represents the weakest aspect of the clinical technique, the emphasis upon a list of unrelated facts under the aegis of a "syndrome." The reader is expected to memorize a series of facts about a condition on the author's say-so, when, in truth, there is a wealth of physiological background on which to rationalize the observations. This is not teaching, but dogma. The book does no credit to the estimable reputation of the author.

SAMUEL P. BESSMAN

IONIC INTERPRETATION OF DRUG ACTION IN CHEMOTHERAPEUTIC RESEARCH.

By Alexander V. Tolstoouhov. Chemical Publishing Company, New York. $10.00. ii + 276 pp.; ill. 1955.

It is difficult to discover just what the author of this book is attempting to discuss. About half of the text is devoted to lengthy verbatim quotations from the work of others, and an appreciable fraction of the space is filled with directions for performing various laboratory tests. The only summary is that on p. 244, where it is concluded that, for drugs that are weak electrolytes: (1) activity depends upon the formation of an undissociated compound between drug and tissue: (2) the compound is the salt of a weak electrolyte; (3) it [illegible], therefore very sensitive to variations in pH; (4) the biological activity can very often be predicted, knowing the isoelectric point of the tissue ampholytes and the dissociation constant of the drug. The dubious nature of these conclusions is apparent.

L. J. MULLINS

BIOCHEMICAL PREPARATIONS. *Volume 4.*

Editorial Board, W. W. Westerfeld, editor in-chief; Eric G. Ball, Herbert E. Carter, Joseph S. Fruton, Arthur Kornberg, Henry A. Lardy, Albert L. Lehninger, Carl G. Niemann, David Shemin, Esmond E. Snell, and Earl W. Sutherland, Jr. John Wiley & Sons, New York; Chapman & Hall, London. $3.75. viii + 108 pp.; ill. 1955.

This volume of *Biochemical Preparations* deals with the reliably checked procedures for the preparation of the following substances of biochemical interest: α-galactose-1-phosphate, homogentisic acid, (dipalmitoleyl)-L-a-lecithin, α-lactalbumin, β-lactoglobulin, alcohol dehydrogenase, inorganic pyrophosphatase, carnosine, L-histidinol dihydrochloride, urocanic acid, N-acetyl imidazole, glycolaldehyde, sodium glyoxylate monohydrate, α-glucose-1-phosphate, tetraacetyl-D-ribofuranose, L-argininic acid, DL-methionine sulfoxide, DL-methionine sulfone and DL-ethionine sulfone, linoleic acid and methyl linoleate, D-glutamic acid. As with past volumes this provides also a very handy and compact reference to the general properties of these compounds.

DAVID B. TYLER

SEMI-MICRO ORGANIC PREPARATIONS.

By J. H. Wilkinson. Charles C Thomas, Springfield. $2.00. x + 94 pp. + 1 pl.; text ill. 1954.

This book describes the equipment and techniques necessary for carrying out organic preparations on a semi-micro scale. The author points out that the semi-micro technique does not mean merely a scaling down of equipment and amounts of materials; new methods and new types of apparatus are called for. Many biologists who use isotopes find that they must carry out their work with small amounts of materials. The apparatus and methods described in this book should be of great value to such workers.

G. R. NOGGLE

THE QUANTITATIVE ANALYSIS OF DRUGS. *Second Edition.*

By D. C. Garratt. Philosophical Library, New York. $17.50. xvi + 670 pp. 1955.

The first edition of this book had the title *Drugs and Galenicals*. This edition has essentially the same structure as the first but with the addition of a number of sections on Aneurine Hydrochloride, Ascorbic Acid, Dicophane, Antibiotics, Steroids, and Vitamin A. In general, those methods that are serviceable or have been reliably checked are selected. A very practical section on the general application of physical methods has been added, as well as new appendices dealing with micro-analytical tests for the presence of extraneous matter in foods and drugs and titration in non-aqueous solvents. This book is highly recommended for all drug analysts.

DAVID B. TYLER

INTRODUCTION TO CHEMICAL PHARMACOLOGY.

By R. B. Barlow; foreword by H. R. Ing. John Wiley & Sons, New York; Methuen & Company, London. $6.25. xiv + 343 pp.; ill. 1955.

This book is primarily designed for chemistry students, particularly those who have received little or no training in biology. Barlow, therefore, includes as an appendix a brief synopsis of anatomy, physiology, and biochemistry. This last seems to be an unnecessary addition to an otherwise good book, which is a first-rate attempt to instruct the students in how structural changes of a compound affect the mechanism of action and, when this mechanism remains unchanged, how intensity of action is affected. Although it is not as elaborately detailed as *Medicinal Chemistry*, the present textbook is comprehensive enough to be admirably suited for a chemistry course dealing with the structure-action relationships of drugs.

DAVID B. TYLER

PARASITOLOGY

MOSQUITOES. *Their Biometrics and Relation to Disease.*

By William R. Horsfall. The Ronald Press Company, New York. $16.00. viii + 723 pp. 1955.

The author states that the objective of this book is "to summarize, as completely as possible, the large and varied literature on mosquitoes that pertains to their biologies and relation to disease." The species are arranged in taxonomic order, and all of the information available, or that which the author considers pertinent, is presented under each species. This information is classified according to five main headings: General, Egg, Larva, Pupa, and Adult; and under these it is subdivided into categories such as Local Distribution, Dispersal, Feeding, Swarming, Mating, and the like. The principal difficulty in this type of compilation seems to me that it becomes necessary to present observations out of context. Isolated bits of information pertaining to a given species are presented, but the reader may miss the reason why such information was obtained in the first place. For example, the response of *Anopheles quadrimaculatus* to lethal temperatures has very little meaning, unless the correlation is made between the responses of this species and its close relatives, and the possible sig-

nificance of such physiologic differences to the distribution and evolution of the entire complex. There is also repetitious treatment of some aspects of mosquito biology and relation to disease under different species. Facts that apply generally to an entire genus are brought out under one species, only to be repeated under another. Thus one finds descriptions of the feeding habits of *Anopheles* larvae under the genus *Anopheles*, and also under *A. crucians*, *A. maculipennis*, *A. punctipennis*, *A. quadrimaculatus*, and several other species.

There will probably be some disagreement with the author on the part of some taxonomists in his use of certain names, especially certain trinomials. One serious taxonomic error is the confusion of *Anopheles pseudopunctipennis* of Oklahoma, Texas, and neighboring states, with *A. franciscanus* of the Far West and Southwest. The reference crediting King and Bradley (1941) with the erroneous distribution is not included in the bibliography. This confusion is apparently responsible for the statement that the distribution of *A. pseudopunctipennis* is "South America to Chile and Argentina up to 2300 m elevation."

Certain statements and tables may lead to misconceptions; e.g., "Persons become infected who expose their bodies to repeated inoculation of sporozoites."

In spite of the above criticisms, the book contains a great deal of information concerning mosquitoes, and will be useful as a short cut to the literature. However, it seems to me that the information would have been far more accessible had it been organized on the basis of biologic and disease categories, rather than according to species.

LLOYD E. ROZEBOOM

BIOLOGY OF *ANOPHELES GAMBIAE*. *Research in French West Africa. WHO Monogr. Ser., No. 9.*

By M. H. Holstein. World Health Organization, Geneva; [Columbia University Press, New York]. $2.00 (paper). 172 pp.; ill. 1954.

This is an English translation of the French edition, which was published in 1952. The English edition contains additional literature references and collection records.

LLOYD E. ROZEBOOM

HEALTH AND DISEASE

ANNUAL REVIEW OF MEDICINE. *Volume 6.*

Edited by David A. Rytand; associate editor, John Anderson. Annual Reviews, Stanford. $7.00. xiv + 459 pp. 1955.

It is not necessary to introduce this book to readers of this journal. In the current volume, MacKay limits his review of endocrinology to three aspects of the subject: the influence of the endocrine system on the nervous system; the nervous control of the endocrine system; and the relation of stress and adaptation of these integrating mechanisms. Belt considers a few of the recent innovations in diagnostic and operative techniques as they apply to diseases of the urinary system, particularly renal homotransplantation, plastic operations for hypospadias, and ureteral transplantations. Scott, in his discussion on recent advances in Infectious Diseases (viruses), limits his review largely to those items that bear directly on the clinical management of such diseases as poliomyelitis, coxsackie viruses, Murray Valley encephalitis, respiratory viruses, the herpes virus group, pox, and infectious hepatitis. Bearn and Kunkel review diseases of the gastrointestinal tract and deal principally with current concepts in liver diseases. Maloney and Blalock deal with those aspects of cardiovascular surgery that are of special interest to the Department of Surgery at Johns Hopkins; the result is a speculative and interesting article dealing with supporting aids in cardiovascular surgery, such as heart-lung machines, hypothermia, arterial homografts, and the management of specific cardiovascular lesions. Valentine's review on Leukocytes and Leukopathies deals primarily with physiology and metabolism. Lepkovsky and Borson, writing on Nutrition and Nutritional Disease, provide an excellent section dealing with obesity and with the current concepts regarding this most serious nutritional disorder. Cooke, Smith, and Skaggs devote a considerable portion of their article on Allergy and Immunology to the consideration of current concepts regarding the cause of the reactions of the allergic phenomena, in particular, the role of proteolytic enzyme systems of the blood (Burdon protease activation theory). Haddow's review is primarily concerned with carcinogenesis, in particular with recent work with hydrocarbons, azo dyestuffs, amines, alkylating agents, and endocrine substances. Critchley concentrates upon clinico-pathological studies in Diseases of the Nervous System, especially the neurological complications of scrub typhus, carcinoma, and diabetes. Stevenson reports on the very little progress that has been made in Psychiatry during the past year. Christie and Bates evaluate the clinical importance of pulmonary function tests; Felson does the same for the newer diagnostic methods of roentgenology, and Sobotka and Carr examine the newer methodology in clinical chemistry. Stecher, in his discussion of rheumatoid arthritis, is concerned primarily with recent data on cortisone, ACTH, and phenylbutazone, while Smirk deals principally with clinically useful drugs for reducing blood pressure, particularly methonium and rauwolfia compounds and combinations. Kirk's paper on Toxicology is chiefly about methodology in isolation, identification, and determination of the new and older toxic agents. Special emphasis is

given to ion-exchange and adsorption and column partition chromatography. Braley stresses the recent developments of ophthalmology diagnosis and treatment. The review of Blattner and Heys is on recent advances in hemophilia, heart disease, toxoplasmosis, congenital malformations, and infectious diseases in children.

DAVID B. TYLER

THE SKIN. *A Clinicopathologic Treatise.* *By Arthur C. Allen. The C. V. Mosby Company, St. Louis.* $25.00. xvi + 1048 pp. + 1 pl.; text ill. 1954.

This is a large book, measuring roughly 12 by 9 by 2 inches, beautifully printed on first-class paper. It abounds in full-page illustrations consisting of original photographs and photomicrographs selected from the author's personal sources.

The author's purpose in writing this book, as avowed in the Preface, was to reduce the barriers between dermatology and other disciplines of medicine. He inferentially deplores the tendency to study disease in artificially broken segments (and, ipso facto, so to practice medicine), by averring that the capacity to study diesase as a whole should comprise one of the great pedagogic aims in medicine. This is a viewpoint which I heartily endorse. The rigidities of disciplines enforced by the speciality boards of medicine have tended to fortify and narrow the specialistic outlook in medical science and practice. *The Skin* is a monumental effort to bring together the knowledge of many specialities, in such a way as to be of help to them all.

There are 28 chapters, grouping the material in pathological categories, etiological when possible, and histopathological otherwise. The pathological presentations are ofttimes stimulating products of independent thought, and give a fresh and challenging look at old but baffling faces. A sturdy champion of his own views, the author uses as his frontispiece a series of 6 interrelated photomicrographs, purporting to show the epidermal origin of melanocarcinoma, a thesis which he has defended for many years against believers in the neurogenic origin. As one might expect from the author's training, much more attention is paid to tumors in this book than in most treatises written by dermatologists. There is a lengthy discussion of melanocarcinoma. The author describes in detail what he has seen in moles and melanocarcinomas which have been excised, but does not tell us very clearly how to classify for purposes of treatment a given mole, or nevus, before it is excised. (He restricts the term "mole" to the intradermal nevus.) He does not help us much with the problem of the individual who so often presents himself with fifteen moles on the face and a hundred and fifteen on the remainder of the body. He does not state that every adult has been said to be entitled to at least fifteen moles by the law of averages, and that the annual incidence of any one of these becoming melanocarcinoma is in the neighborhood of 0.0001 per cent. He advises only that, when feasible, moles (nevi) on the palms, soles, and genitalia should be prophylactically excised. One is likely to get an unjustifiable terror of a mole (nevus) from this book.

The clinical discussions leave something to be desired, particularly in so far as the clinical treatment of disease is concerned. Notably, there is a tendency to emphasize forms of treatment which theoretically should prove beneficial, but practically have been found wanting. As an example I cite the advocacy of the use of estrogenic hormones in the treatment of acne vulgaris. Although there is little doubt that an androgenic influence on the cutaneous sebaceous apparatus is a prime factor in the causation of acne vulgaris, at the same time it is just as certain that up to the present moment attempts to cure or even benefit patients with acne vulgaris by the administration of estrogens, or other androgen-influencing hormones, have failed. While advocating the use of hormones, Allen condemns treatment with x-rays, a therapeutic tool which has had wide acceptance for many years. This is probably a reflection of the viewpoint of many radiologists, whose discipline teaches that no one except a radiologist knows how to use such radiations.

The hindsight in this book is much better than the foresight, but such will always be the case when pathologists essay to describe the clinical manifestations of disease, as well as the pathological ones; and to delve into clinical management and treatment, a realm where they are relatively inexperienced. Nevertheless, this review ought not to end upon a note of small criticism. Here is a book which is indeed great, and which will be an illuminating source of knowledge for physicians and students.

H. HANFORD HOPKINS

PRINCIPLES OF INTERNAL MEDICINE. *Second Edition. Edited by T. R. Harrison, Raymond D. Adams, Paul B. Beeson, William H. Resnik, George W. Thorn, and M. M. Wintrobe. The Blakiston Company, New York; [The McGraw-Hill Book Company, New York].* $16.00, one vol. student ed.; $20.00, 2 vol. professional ed. xxiv + 1704 pp. + 88 pp.; ill. 1954.

Currently the two textbooks of medicine most used in this country are (1) that of Cecil and Loeb (Saunders, ninth edition, 1951) and (2) the book which is the object of this review. It is interesting to contrast the two. Cecil and Loeb is conventional in its approach and organization. It embarks on the very first page upon a discussion of infectious diseases, specifically those due to viruses, and proceeds uninterruptedly through all the categories of disease up to (and including) the

psychoses. Each disease entity is discussed under such categories as definition, etiology, pathological anatomy and physiology, symptoms, differential diagnosis, treatment, and (at times) prevention. Although the two textbooks are of roughly the same length, Harrison's book devotes its first third to a discussion of general topics before any specific disease is discussed in any detail. The types of general topics discussed are demonstrated by the following examples: pain, fever, shortness of breath, jaundice, depression, delirium, inheritance of disease, general care of the patient. Obviously the idea in the case of Harrison's text is that the patient does not present himself as a "case" of this or that disease but rather as a victim of one or more cardinal manifestations which must be evaluated against the background of the total clinical picture before accurate diagnosis and effective therapy can be achieved. There is much to be said for this point of view. On the other hand, the discussion of individual disease entities is, in most cases, more detailed in the Cecil and Loeb. The technique used by Harrison and his collaborators of discussing general principles does remove the necessity of a certain amount of repetitiousness in discussing separate entities. The difference of approach is somewhat more apparent than real, inasmuch as each major section in the Cecil and Loeb book, e.g., those on infectious diseases and diseases of the cardiovascular system, is introduced by a survey of general principles.

Actually both of these books are excellent. Their popularity is well justified. Practically speaking, the differences in presentation probably make no great difference. Either will be excellent for students. As a gross generalization, I would guess that Harrison's would be better for the student being introduced to medicine and the Cecil and Loeb better for a practitioner, who must have a comprehensive, systematic 1-volume treatise for ready reference.

VICTOR A. MCKUSICK

OUR DAILY POISON. *The Effects of DDT, Fluorides, Hormones and Other Chemicals on Modern Man.*

By Leonard Wickenden; foreword by Morton S. Biskind; preface by William Coda Martin; introduction by Jonathan Forman. The Devin-Adair Company, New York. $3.00. xiv + 178 pp.; ill. 1955.

In this book Wickenden has discussed extensively some current practices in agriculture, public water supply, animal husbandry, etc., which have the potentiality of becoming public hazards. Some of the problems presented are the artificial fluoridation of drinking water, the increasing use of pesticides, the use of hormones in poultry and beef fattening and in the cosmetic industry, and the use of bleaches in the flour industry.

In our age of outstanding scientific discoveries one danger is that preliminary scientific information will be looked upon as a final solution to a problem. The medical profession, frequently made aware of this danger, is justifiably cautious in the use of new drugs and pharmaceuticals. This, unfortunately, is not true in other fields of human endeavor, as described in this volume. It is undoubtedly true that we should attempt to learn as much as possible concerning the influence of small doses of pesticides, fluorine, hormones, etc., before these materials are used extensively. It seems unnecessary, however, for the author to stress that 274,844,000 pounds of pesticides were produced in 1951 and that this is sufficient to kill 15,200,000,000 people. Such figures, while startling, are essentially meaningless. In spite of such shortcomings, this book should be of considerable interest to readers of varied scientific backgrounds.

R. VAN REEN

ION EXCHANGE AND ADSORPTION AGENTS IN MEDICINE. *The Concept of Intestinal Bionomics.*

By Gustav J. Martin. Little, Brown & Company, Boston and Toronto. $7.50. vii + 333 pp. + 5 pl.; text ill. 1955.

This book is based on the generalization that all chronic degenerative diseases have an important etiologic component—the adsorption of small quantities of toxic materials from the intestine—and that this adsorption can be prevented by the use of suitable ion-exchange resins and adsorption materials which can condition the gut to retain toxic substances but still permit beneficial nutrients to enter the system.

DAVID B. TYLER

NEWER CONCEPTS OF THE CAUSES AND TREATMENT OF DIABETES MELLITUS. *Proceedings of the Symposium on Diabetes sponsored by the New York Diabetes Association and held at Memorial Hospital and The New York Academy of Sciences, New York City, October 8, 1953.*

The National Vitamin Foundation, New York. $2.50 (paper). iv + 181 pp.; ill. 1954.

DISEASES OF THE LIVER.

By Mitchell A. Spellberg. Grune & Stratton, New York. $16.50. x + 646 pp. + 2 pl.; text ill. 1954.

Spellberg has written this book for clinicians. He emphasizes throughout "three spearheads for diagnosis: clinical, biochemical, and anatomic facets." The book is profusely illustrated with diagrams and photographs, and each important subject is summarized briefly in boldface type to recapitulate the highlights. A clear picture of liver function tests, covering interpretation and value, is given. Pigment tests are care-

fully discussed, but the treatment of carbohydrate and protein metabolism is skimpy. An excellent and up-to-date chapter on needle biopsy of the liver is given. The relevant anatomy and physiology concerning each disease are discussed, and recent concepts are included. Specific chapters are devoted to jaundice, neoplasms, toxins, infections, cirrhosis, endocrine effects, and errors of metabolism. Detailed references both specific and general, are included in a bibliography of 2400 items. Although there are no specific sections devoted to treatment, the therapy of each condition is developed after the deranged metabolism is presented, for, as observed in the Preface, "intelligent recognition and treatment of diseases of the liver can be accomplished only by looking for the cause and the resulting physiologic and anatomic alterations." Detailed tables of differential diagnosis are included for each condition.

Spellberg succeeds in his purpose of producing a clinical textbook for clinicians. The view is fresh, the presentation is abreast of current research in the field, and the expression is didactic. It will be of value to medical students, to practitioners of medicine, and to anyone interested primarily in the clinical aspects of liver disease.

FRANK L. IBER

LIVER INJURY. *Transactions of the Twelfth Conference September 21, 22 and 23, 1953, Princeton, N. J.*

Edited by F. W. Hoffbauer. The Josiah Macy Jr. Foundation, New York. $4.25. 232 pp.; ill. 1954.

This volume records the interchange of ideas of a number of eminent scientists upon 4 topics of general interest in relation to liver disease. Einar Lundsgaard presented a review of liver and carbohydrate metabolism and included some of the recent investigations of his laboratory. Samuel Gurin and H. W. Kosterlitz reviewed respectively fat and protein metabolism with regard to the liver, and W. Stanley Hartroft reviewed the recent Toronto work on cardiovascular lesions in choline-deficient rats.

This volume holds particular interest for all who are concerned with the biochemistry or physiology of the liver or with the investigation of liver disease. The presentations, by men participating in the field, are authoritative and the members of the conference were persevering and penetrating in their discussion. Many new facets for future investigation are to be found in this small volume. The investigators seemed to catalyze thinking in one another, so that this report is not only thought-provoking, but also provides delightful reading.

FRANK L. IBER

PROBLEMS AND CONTROL OF AIR POLLUTION. *Proceedings of the First International Congress on Air Pollution held in New York City, March 1–2, 1955 under the sponsorship of the Committee on Air-Pollution Controls of The American Society of Mechanical Engineers.*

Edited by Frederick S. Mallette. Reinhold Publishing Corporation, New York; Chapman & Hall, London. $7.50. vi + 272 pp.; ill. 1955.

In this collection of 25 contributions from the United States, England, Canada, the Netherlands, Portugal, Italy, and France, emphasis is placed almost exclusively on the problem of the industrial pollution of air and on such means as are available for its identification, measurement, and control. Other forms of air pollution are noted only in a single chapter, e.g., by pollens and other allergens (3 pages), radioactive debris (8 lines), and pathogenic bacteria and viruses (listing only). The industrial orientation of this symposium may depend on the facts, which are brought out clearly in the book, (a) that the history of air pollution as a recognized problem goes back some 700 years, but that as yet hardly any community or country has done more than study it and make recommendations (St. Louis and Pittsburgh are noted as exceptions); and (b) that it is only with pollutants detectable by the senses (visible, like smog, or irritating or malodorous) that a cause-and-effect relationship seems sufficiently obvious to arouse public indignation, which itself seems to be required before any action can be hoped for. That portion of the air-pollution problem with which the book deals is presented clearly and effectively; and since some of the papers will interest the general reader, while others will be useful to specialists, the book should be in the hands of all who are concerned with the over-all problem. This category presumably ought to, but evidently does not, include virtually everybody. An index would have been helpful.

T. ROSEBURY

AIRBORNE CONTAGION AND AIR HYGIENE. *An Ecological Study of Droplet Infections.*

By William Firth Wells. Published for the Commonwealth Fund, Harvard University Press, Cambridge. $6.00. xxxii + 423 pp.; ill. 1955.

Present-day concepts of air-borne infection recognize two separate means for the spread of infective particles: (a) via contaminated dust, which settles out rapidly but may be raised again by air currents or the movement of dust-laden objects like blankets; and (b) via droplet-nuclei—the infective residue of minute droplets which are atomized from the respiratory passages, and which, drying almost instantaneously in unsaturated air, are small enough to remain suspended indefinitely. A third mode of spread, via larger droplets that settle in a well-defined trajectory within a few feet from their source, as in a sneeze, although formerly considered of paramount importance, is now recognized as statistically unimportant compared with the other two. Indeed,

explosive epidemics of air-borne illness and those found to be carried over considerable distances or from one floor of a building to another, can be accounted for best, perhaps only, by mechanism (b). The experimental basis for these concepts is now well established. William Firth Wells is an acknowledged pioneer in such experimentation. He is the author of the term "droplet-nucleus" and is largely responsible for the experimental impetus that has given it significance. His book is an account of his studies in this field during more than twenty years.

The book would in consequence be expected to be a definitive expression of the subject, in which the author's own researches, detailed in more than 70 items in the bibliography, would be set in perspective both to the historical development and to the present status of the whole field. While this seems to have been the author's objective, he has not achieved it. The work suffers from poor organization, and fails to reach acceptable standards of coherence or clarity either in its verbal content or in its numerous charts, tables, and photographs, many of which are over-complex or inadequately labeled. A few representative examples may be given. The author's handling of the important matter of atomization does not include a development of the principles of the subject, nor are the details given clearly of the author's own atomizers, either in text or in illustrations. The procedure used by Wells for an estimation of dosage of experimental animals exposed to air-borne tubercle bacilli (pp. 112–118) is not clearly defined. On p. 33 the author rejects rather summarily, as "giving illusions of efficiency," the air-sampling device adopted in the wartime group with which the reviewer was associated. Wells fails to take note of performance data given in the report of that study, or of confirmatory data in a subsequent independent study (Kluyver and Visser, 1950, *Leeuwenhoek ned. Tijdschr.*, 16: 229). Moreover, although he dismisses the method, Wells later (p. 129) accepts the data obtained with its aid, without explaining the inconsistency thereby entailed.

Although this book is said to be intended for the layman as well as for the specialist, it seems unlikely that the former group could derive much enlightenment from it. Students of air-borne infection will need to consult it; but the best I can find to say of the book myself is that its contents will need to be considered when an adequate treatise on the whole subject of air-borne infection comes to be written.

T. ROSEBURY

CEREBRAL PALSY, *its Individual & Community Problems.*

Edited by William N. Cruickshank and George M. Raus. Syracuse University Press, Syracuse. $7.50. xvi + 560 pp.; ill. 1955.

In this book an attempt is made to cover in a single treatment the medical, psychological, therapeutic, and social points of view of a very complicated condition with the hope of improving the inter-profession understanding of the problems in the habilitation of cerebral palsied persons. It is an excellent work, well organized, clearly presented. The psychologist, educator, speech pathologist, and social worker will find this volume an excellent reference to the medical aspects of the problem, the therapeutic procedures that may be used, and the educational and counseling problems both for patient and parent as well as the entire community. The contributors to this book are educators, psychologists, therapists, and physicians who have had considerable experience and so acquired a first-hand knowledge of the problems they deal with.

DAVID B. TYLER

INTERPRETATION OF SCHIZOPHRENIA.

By Silvano Arieti. Robert Brunner, New York. $6.75. xvii + 522 pp. + 4 pl.; text ill. 1955.

The dust jacket of this book has some very flattering comments from many well-known psychiatrists and a psychologist, as well as one who is, I am sad to state, a neurophysiologist of talent and promise. David Rioch thinks that ". . . Dr. Arieti's formal analysis of schizophrenia thinking and communication" is of particular interest, especially that "his differentiation of conventional logic and the paleo-logic of schizophrenia provides a clearly stated guide for interpreting the individualistic communication of patients, who until recently were classed as demented." Potter believes this to be the "most comprehensive presentation of the subject since that of Bleuler." Five other critics are even more flamboyant in their praise. I cannot seem to get into the proper frame of mind to join them in their joy, a fact, I am sure, that will not interfere with the sale of this book or make unbelievers out of any of the faithful of this cult.

Once upon a time, Kraepelin, as surely everybody knows, after careful observation of thousands of patients, was able to discern, among what appeared to be grossly dissimilar cases, a common characteristic, namely, that some of them show a progressive decay in personality until a state of permanent dementia is reached. These patients he called the praecox group, to separate them from the manic-depressives who did not have this tendency toward deterioration. He thought that this condition (dementia praecox) was due either to an organic pathology of the brain or to a metabolic disorder. Arieti urges us not to be too harsh with Kraepelin for daring to propose this view. After all, he says, "Nobody would criticize Galileo for not knowing the principles of electricity." Why, therefore, should we in view of our present knowledge sniff at old Kraepelin, who only had the cross section, and not the longitudinal

view of the certain types of demented fellows. Furthermore, he was concerned only with the mechanism of the patient's thinking, not with his thoughts. And after all —what could you expect from one who knew little or nothing of the principle of teleologic regression, Von Domarus' principle, Nunn's concept of hormic energy, or Freud's unconscious (as collectivized by Jung)? Therefore, to correct the misconceptions brought on by Kraepelin and which are still held by some very odd fellows, this book was developed. The conception developed here is that schizophrenia is a delayed psychological response occurring later in life in an individual who was exposed to an extreme state of anxiety in childhood. If you accept this premise, buy the book —but be tolerant of poor old Kraepelin.

DAVID B. TYLER

PSYCHOLOGY AND ANIMAL BEHAVIOR

SPIRIT AND NATURE. *Papers from the Eranos Yearbooks. Bollinger Series XXX - 1.*

By Ernesto Buonaiuti, Friedrich Dessauer, C. G. Jung, Werner Kaegi, C. Kerényi, Paul Masson-Oursel, Fritz Meier, Adolf Portmann, Max Pulver, Hugo Rahner, Erwin Schrödinger, and Walter Wili. Pantheon Books, New York. $5.00. xvi + 492 pp. + 3 pl. 1954.

Since 1933, noted scholars and scientists from various European countries have met in Ascona, Switzerland for annual conferences during August. They have lectured on some central theme, and each participant has contributed from his particular field of knowledge and experience. The lectures were published in the original language, usually German, as "Eranos Jahrbücher." This book is the first of a planned series of 9 or 10 volumes, presenting selections from these yearbooks translated into English. The central theme of this first volume is Spirit and Nature; its goal is to bridge the distance between the humanities on the one hand and natural science and technology on the other.

Both the opening and the closing chapters of this book are contributions by C. G. Jung, the Spiritus rector and wise old man of the Eranos meetings. As usual, Jung has much to say concerning his themes, and I can only try to point out some of the important statements and conclusions. The phenomenon called "spirit" has certain hallmarks: spontaneous movement and activity; spontaneous capacity to produce images independently of sense perception; and the autonomous and sovereign manipulation of these images. The psychic manifestations of the spirit are of an archetypal nature; i.e., they depend on an autonomous, primordial image which is universally present in the preconscious make-up of the human psyche. Man has conquered spirit as well as nature, but without realizing what he is doing. Man's worst sin is his unconsciousness. The original title of this first lecture was The Psychology of Spirit; in a significant variation the second paper of Jung's, the last chapter in this volume, is named The Spirit of Psychology. Here the author is undertaking a more general analysis of the nature of psyche and the empirical concepts concerning it. The "psyche proper" extends to all functions which can be brought under the influence of a will—"freedom of choice." The will cannot coerce the instinct nor has it power over the spirit. Spirit and instinct are by nature autonomous Where instinct predominates, "psychoid" processes set in which pertain to the sphere of the unconscious as elements incapable of consciousness. There is an unconscious in the sense of William James' "fringe of consciousness," and the Freudian personal unconscious belongs to this marginal phenomenon; also, the unconscious in this sense is not simply the unknown, but the unknown psychic, in an extremely fluid state of affairs. Psychic processes seem to be balances of energy flowing between instinct and spirit. They behave like a scale along which consciousness slides.

There is, however, a psychic unconscious which is not "a fringe of consciousness and not personal"; it comprises archaic vestiges and primitive modes of functioning which meet the instinct in the biological conception of the "pattern of behavior." Archetypes are not inborn ideas, but typical forms of behavior; and quite likely a man is born with a specifically human mode of behavior. The chapter closes with interesting remarks on certain similarities and dissimilarities between psychology and physics. The analogies, however, do not prove an already existing connection.

Of the remaining 11 papers only 3 were given by representatives of the natural sciences: the physicists, Erwin Schrödinger (The Spirit of Science) and Friedrich Dessauer (Galileo and Newton: The Turning Point in Western Thought); and the biologist, Adolf Portmann (Biology and the Phenomenon of the Spiritual). Each one of them, paying tribute to the spirit in his own way, emphasizes the common ground of all scientific endeavor.

Schrödinger describes spirit in a broad sense as "this ego that consists of thought." He then proceeds "to put his listeners as subjects" in contact with those currents of thought which in his opinion have determined the development of science in the last one hundred years. He sees the basic force, the basic trend in the increasing simplification and unification of our view of the universe. The endeavor to disclose a common, homogeneous basis in all the broad realms of scientific research is today particularly relevant, because this basic motif is itself simplification and unification. The discontinuity or discreteness of matter has been extended—200 years after it had been surmised for matter—to force also, since Einstein proved that energy equals mass. The significance of this idea is that discrete things can be counted with the help of the clearest mathematical equipment, the integer. Indeed, the universality of

discreteness appears to show that the method of enumeration, the method of the integer, is really "the royal road, the only road by which we may hope to achieve real insight."

Friedrich Dessauer holds that the divergence, even conflict, between the two great realms of science is not necessarily inherent in the subject matter, but rather in human attitudes, which are historically determined. Certainly, there were two epochs, the one represented by Galileo, the other by Newton, in which man was still contemplating the world "uno aspectu." Yet this old sentiment was almost lost when natural science was thrust out of the sphere of spiritual unity, and the investigation of nature was no longer regarded as a search for the revelation in all man's intellectual endeavor; and everything that the natural scientist discovers in his field is spiritual, for it is rule, order, law.

Adolf Portmann's paper is informative and lucid. The biologist encounters "spirit" particularly as that quality in man's work and life that distinguishes him from even the highest animal. Biologists call the special mode of existence "inwardness." This "spirit" is fully at work in every stage of human development; man does not become human only in the last stage. Nor can we evaluate our human specificity as deficiency, as regression; biological investigation reveals it as directly productive and independent on the basis of special characters which belong only to this biological system and are lines in a relatively closed system.

The remaining chapters in this volume are devoted to the vision, the formulation, and the changing of conceptions of "spirit" in different cultures and historical epochs.

Fritz Meier gives what appears to be a most learned investigation on The Problem of Nature in the Esoteric Monism of Islam. Paul Masson-Oursel's lecture on The Indian Conception of Psychology and Indian Techniques of Salvation was taken from an earlier Eranos Conference (1937).

C. Kerényi (Apollo Epiphanies) speaks of some manifestation of the spirit in Greek mythology. There the realization of knowledge, the blowing of the spirit, the epiphany seem bound up with Apollo. He is the god of light driving the powers of darkness from his temple. Yet, as Jung's archetypal figure of the spirit, he has also a very sinister aspect, clearly expressed in the two instruments so closely related to him, the arrow and the snake.

Walter Wili discusses the History of the Spirit in Antiquity from the pre-Socratic philosophers to Posidonius and his most influential proponent Cicero.

Max Pulver writes about the Experience of the Pneuma in Philo; Hugo Rahner, S.J., investigates the Earth Spirit and Divine Spirit in Patristic Theology. We find interesting remarks about dreams and their nature, and about the power of psychological analysis in the patristic literature.

Ernesto Buonaiuti (Ecclesia Spiritualis) outlines the Iranian (Zoroaster), Greek, and Biblical precursors of the Spiritual Church and follows the Ecclesia spiritualis through the 20 centuries of Christian tradition.

Werner Kaegi (The Transformation of the Spirit in the Renaissance) disagrees with much that has been written on this subject by Jacob Burckhardt and by Michelet and their followers. It is not the "awakening of the individual," though an important insight resided in this catch-word of Burckhardt's, that characterizes the Renaissance and only the Renaissance. Its true spiritual element is to be found in the striving to find a new simplicity. Nature and spirit are seen in a relation of harmony, rich in correspondences, as two aspects of the same being.

The tenor of the papers in this first volume is evocative rather than dogmatic. The (uncommon) name "Eranos" was suggested for these gatherings of eminent scholars by the late theologian of Marburg, Rudolf Otto. It designates a kind of informal symposium to which each guest contributes his share. Quite likely, then, the reader of this book is deprived of the most stimulating feature of the actual Eranos meetings, the evening discussions in the garden above the Lago Maggiore. The value of the book is enhanced by an extensive index, by brief biographical notes on the various authors, and by a list of the contents of the Eranos Yearbooks from 1933 through 1953.

WALTER O. JAHRREISS

HUMAN BIOLOGY

YEARBOOK OF ANTHROPOLOGY. *Volume One, 1955. Edited by William L. Thomas, Jr. Wenner-Gren Foundation for Anthropological Research, New York.* Not for sale. xvi + 836 pp. + 1 pl. 1955.

This handsome and massive volume, dedicated to Dr. Axel Leonard Wenner-Gren, is a fitting successor and companion volume to *Anthropology Today: An Encyclopedic Inventory*, which was issued by the same Foundation in 1953. "With this volume the Wenner-Gren Foundation inaugurates a new series of publications, intended to increase its service to the anthropological profession with which it has become affiliated. The purpose of the present work is to report on the major accomplishments and trends during 1952–1954 in aspects of the field of anthropology." (p. vii).

After an Editor's Preface, the volume opens with a guest editorial by Julian S. Huxley, on Evolution, Cultural and Biological, which bridges the gap, if there is one, between the biological and the cultural, and is delightful and illuminating reading. The subsequent parts of the book deal with: Man's Past: Environments, Relics, Ancestors; Other Considerations of Theory; Practical Affairs; Regional Round-up: Europe and

Southwest Asia Anthropological Reviews for 1952–1954; Reference Data. The Regional Round-Up is a description, by specialists in the respective regions, of recent anthropological work in Southwest Asia, Greece, Italy, Spain and Portugal, France, The Netherlands and Belgium, Great Britain and Eire, Scandinavia, Deutschland, Austria and Switzerland, Yugoslavia, Eastern Europe, the Union of Soviet Socialist Republics. The section called Reference Data, to which 72 pages are devoted, includes Dissertations in Anthropology Submitted to Educational Institutions of the World in Partial Fulfillment of the Requirement for the Ph.D. Degree or its Equivalent; Medals, Awards, and Memorial Lecture Series in Anthropology; Professional Associations; International Organizations of Anthropological Interest; and National and Regional Professional Associations of the United States of America. There is a 63-page index. For the anthropological world, this volume is a landmark.

A high level of competence is maintained throughout. It is not possible to list here all of the contributors and their subjects of discourse, much less to review these contributions. Selection is invidious, and can merely reflect the interests or predispositions of the reviewer. I found especially interesting and valuable the following contributions: Fossil Man and Human Evolution (Loren C. Eiseley); Archeological Theories and Interpretations (Emil W. Haury); History of Anthropological Thought (A. L. Kroeber); Anthropology and Human Relations in Business and Industry (F. L. W. Richardson, Jr.); and Great Britain and Eire (W. C. Brice). If space permitted, others would be added, for the volume is a mine of information, interpretation, and reference material. It is an anthropologist's dream-book and more. The Wenner-Gren Foundation, which has for many years assisted anthropological research, has again placed the profession under great obligation to it.

WILSON D. WALLIS

ANTHROPOLOGY.

By J. E. Manchip White. Philosophical Library, New York. $2.75. 191 pp.; ill. 1955.

This book, written for those who are not anthropologists, is a pleasure to hold and to read. The exposition is lucid and the author's knowledge of the subject adequate for the topics treated: physical, cultural, social, and applied anthropology. Professional persons will be squeamish over some of the slips or presumptions taken in the physical anthropology. For example: what is the evidence for the statement that at the time *Dryopithecus* "appears to have been firmly established as an upright-walking ground-dweller there were also marked signs that it was at this stage of his evolution that man adopted a definitely omnivorous diet"? Or, for that matter, that there *was* a man to scan the menu? How can "the impression on the inner surface of the cranium suggest that the pattern of the brain was a little different from our own"? The use of the fluorine test is inadequately described.

I have listed 14 other items which I think any professional anthropologist would challenge. Nearly all of them are in the field of physical anthropology. Few, if any, of these, I think, will do the non-professional person much intellectual damage. In fundamental matters the author, it seems to me, has done well by his subject.

WILSON D. WALLIS

CULTURAL ANTHROPOLOGY. *An Abridged Revision of Man and His Works.*

By Melville F. Herskovits. Alfred A. Knopf, New York. $6.50 (trade ed.); $5.00 (text ed.). xviii + 569 + xxxiv pp. + 16 pl.; text ill. 1955.

As stated in the Preface, this is an "abridgement and revision of *Man and His Works*," with some "changes in organization." With that compression it has been possible to include most of the important materials of the previous edition and all of the principal topics. An outline of physical anthropology is given, but the main emphasis is on cultural aspects.

WILSON D. WALLIS

THEORY OF CULTURE CHANGE. *The Methodology of Multilinear Evolution.*

By Julian H. Steward. University of Illinois Press, Urbana. $4.00. viii + 244 pp. 1955.

This is a book which students of culture will not want to miss. It brings together, in expanded and revised form, both papers published elsewhere and some which appear here for the first time. Particularly valuable are the studies of ecology in relation to culture, and the analyses of patrilineal bands in various parts of the world. While not playing down the importance of diffusion, the author believes that independent, or parallel, developments have been important in many patrilineal societies.

The author devotes a chapter to evolution but does not tell us what he means by this term, except in the astonishing sentence: "Culture evolution may be regarded either as a special type of historical reconstruction or as a particular methodology of approach." He speaks of "the fruitless assumption that culture comes from culture"; and on the following page assures us that "Cultures do, of course, tend to perpetuate themselves." One must congratulate the publisher on a very attractive format.

WILSON D. WALLIS

STANDING ROOM ONLY. *The Challenge of Overpopulation.*

By Karl Sax. Beacon Press, Boston. $3.00. viii + 206 pp.; ill. 1955.

It has been proposed, as a solution for the difficult population density problems confronting Puerto Rico, that birth control legislation be immediately enacted and made retroactive to 1895. That, I am sure, would be a combination of positive and preventive checks that is so sorely needed in many of the areas of this sick world. In this book Karl Sax very frankly discusses the opposition of both the Catholic Church and Moscow. It is true that both of these States have much in common on this question, as their spokesmen have done much to encourage high birth rates and to prevent the enactment of laws that would freely distribute information about modern contraceptive techniques. I feel, however, that the accusing finger must also point, and much more sharply, at *man* himself. Devout Catholics practice birth control undoubtedly as frequently as unbelievers. They have small or large families depending on their personal feelings in the matter, and my own impression is that the personal feelings of the great majority of men and women are to have children, regardless of the problems that having them creates. And they usually do have them, church or no church, Malthus or no Malthus, and in sufficient numbers to give no escape from the laws of population growth; and so it is necessary to invoke the positive checks every couple of decades or so. Given a choice of which of those two checks he desires—high death rates or low birth rates—it is fairly certain that man will vote for the latter—for the other fellow.

DAVID B. TYLER

DOWN TO EARTH. *A Practical Guide to Archeology.*

By Robin Place. Philosophical Library, New York. $7.50. xvi + 173 pp.; ill. 1955.

Those who have a penchant for the past, and particularly for Britain's past before the period of written records, will find here much to satisfy him, in text and illustrations, each of which is of high order. The subtitle, A Practical Guide to Archeology, is appropriately descriptive. The reader is not merely told about objects, places, and inferences, he is taken to the sites, hears the plans for excavation, witnesses the digging, and overhears the discussions and conclusions regarding the significance of the discoveries.

Women seem to have been more outspoken about their age in the Old Stone Age than in the automobile age, if we are to judge by evidence from a Derbyshire cave. There was found "the skull of a woman of fifty-two, a venerable age for those times." The precision of that diagnosis of age would be quite remarkable for our times, and is something of a strain on one's anatomic conscience, if he has one. At one point the reader will have some difficulty in understanding whether it is the Pleistocene Period or the Paleolithic Period which ended in 7900 B.C. Later reference to this year refers to it as "the end of the Paleolithic, and beginning of the Mesolithic Period." One wonders. Even for a given locality, can geological evidence give more than a maximum age for its post-glacial culture?

WILSON D. WALLIS

THE MATERIAL CULTURE OF PUEBLO BONITO. *With Appendix: Canid Remains from Pueblo Bonito and Pueblo Del Arroyo. (Pub. 4172). Smithson. misc. Coll., Vol. 124 (whole vol.). Published under a Grant from the National Geographic Society.*

By Neil M. Judd; appendix B by Glover M. Allen. The Smithsonian Institution, Washington. $5.00 (paper). xii + 398 pp. + 101 pl. + 1 folded map; text ill. 1954.

This volume, in preparation for 25 years, is a fitting monument to its author. As Gilbert Grosvenor, President of the National Geographic Society, has stated in the Preface: "Mr. Judd's painstaking investigations have made it possible to reconstruct to a remarkable degree the everyday life and culture of the people who lived in Pueblo Bonito approximately 1,000 years ago . . . and attained their 'Golden Age' after A.D. 1000. . . . Some three hundred years before the coming of Columbus they disappeared without a trace." Their cemetery, if they had one, has not been found, and only scant skeletal material has been recovered on the site. The appended study by an authority on aboriginal New World dogs, the late Glover M. Allen, is based on remains of red fox, gray fox, at least 30 coyotes, and about 12 Indian dogs. "The dog bones are nearly all of one type, representing a medium-sized dog with slender muzzle and high, elevated forehead, apparently the same as I [Allen] have . . . called the Plains Indian dog, or perhaps the long-haired breed." A skeleton of one dog differs so much from those of the other dogs that "it might be thought to represent another breed of dogs." Separate chapters deal with Subsistence and Living Conditions; Dress and Adornment; Household Tools and Utensils; Pottery; Implements of the Field and Chase; Objects of Religious Implication; Intramural Burials; and Navaho Notes from Chaco Canyon. There is a bibliography and an index.

WILSON D. WALLIS

THE JAKETOWN SITE IN WEST-CENTRAL MISSISSIPPI. *Anthrop. Pap. Amer. Mus., Vol. 45, Pt. 1.*

By James A. Ford, Philip Phillips, and William G. Haag. American Museum of Natural History, New York. $3.00 (paper). 164 pp. + 2 charts + 8 pl.; text ill. 1955.

This site, about three miles from Belzoni, is happily and archeologically named, recording the alleged fact that it was so called "because during Prohibition days the people there drank a lot of Jamaica ginger [sic] and had quite a few cases of paralysis that was known as 'Jake-leg.'" The respective dates assigned the materials by geologic evidence based on former river channels and deposits, and by radiocarbon analysis of charcoal fragments, are at variance. The former suggests a date of about 1500 B.C., the latter a date of between 300 B.C. and 500 B.C. Animals represented by the bones include: deer, birds of various small species, snapper turtle, fishes, including garpike, terrapin, turkey, raccoon, bear, probably black, opossum, rabbit, and miscellaneous small mammals, including mink, marten, bobcat, beaver, and squirrel.

WILSON D. WALLIS

SOCIAL STRUCTURE AND CULTURE CHANGE IN A LEBANESE VILLAGE. *Viking Fund Publ. Anthrop., No. 21. By John Gulick. Wenner-Gren Foundation for Anthropological Research, New York.* $3.50 (paper). 191 pp. + 9 pl.; text ill. 1955.

Here we visit a near-coastal village in what is possibly one of the longest continually inhabited regions of the world. Its ecology is briefly described, and the physical aspects of the village, al-Qurni, about midway between Tripoli and Beirut. Main stress is laid upon the social structure of life in the village, and its principal institutions. One who reads this account carefully should soon know how to feel at home in al-Qurni, or at least should know how the inhabitants feel at home in it. They have adopted many Western European, more specifically, American, culture traits, principally material objects. As throughout the world, it is the young people who welcome change most ardently.

In spite of many borrowings, of ideas as well as of things, the culture remains fundamentally and thoroughly Arabic, with a special fondness for that language and literature. Nine pages of plates enable one to visualize many physical aspects of the community. This is an informative, entertaining, and revealing monograph.

WILSON D. WALLIS

SONGS AND STORIES OF THE CH'UAN MIAO. *Smithson. Misc. Coll., Vol. 123, No. 1. Pub. 4139. By David Crockett Graham. Smithsonian Institution, Washington.* $4.00 (paper). xii + 336 pp. + 24 pl. 1954.

The Ch'uan Miao is an ethnic group related to the Chinese but differing from them in certain physical features and in language and customs. About 150,000 of them live in the far Southwest, particularly in the provinces of Szechuan and Kweichow. Their collected legends, historical and ethnological tales, songs and chants, stories, and folktales throw an interesting light on their place in the great Mon-Khmer family and their partial assimilation of Chinese ways and manners while for the most part maintaining their own language and traditions.

DE OMNIBUS REBUS ET QUIBUSDAM ALIIS

SCIENCE IN PROGRESS. *Ninth Series. Edited by George A. Baitsell; contributors, Laurence H. Snyder, Curt Stern, E. J. Kraus, Kenneth V. Thimann, George Wald, Lee E. Farr, Felix Bloch, William Shockley, D. B. Steinman, and Wallace R. Brode. Yale University Press, New Haven; Geoffrey Cumberlege, Oxford University Press, London.* $6.50. xviii + 343 pp.; ill. 1955.

Once again an outstanding collection of scientific publications based on the National Sigma Xi Lectures has been presented to the scientific and lay public. This, the ninth series, is composed of the following essays: Present Knowledge of Human Heredity, and Its Applications, by Laurence H. Snyder; Two or Three Bristles, by Curt Stern; The Significance of Ergocrines in Agricultural Practice, by E. J. Kraus; The Physiology of Growth in Plant Tissues, by Kenneth V. Thimann; The Molecular Basis of Visual Excitation, by George Wald; The Impact of Nuclear Science on Medicine, by Lee E. Farr; Nuclear Magnetism, by Felix Bloch; Transistor Physics, by William Shockley; Suspension Bridges: The Aerodynamic Problem and Its Solution, by D. B. Steinman; and Color and Chemical Constitution, by Wallace R. Brode.

Readers familiar with the series will find in this volume the same degree of excellence characterizing its predecessors. Those who have never become acquainted with the series should take this opportunity to examine a particularly well-balanced and authoritative volume.

V. G. DETHIER

THE SCOURGE OF THE SWASTIKA. *A Short History of Nazi War Crimes. By Lord Russell of Liverpool. Philosophical Library, New York.* $4.50. xii + 259 pp. + 14 pl. 1954.

For those readers of this journal who have forgotten the story of Hitler's Germany—the 12,000,000 murders by the cold calculating beasts who paraded as men; and for those who think we should once again be pals with a nation that attempted to destroy races and the very existence of free states, this book is recommended with its photographs and documents. It is a most formidable and unforgettable indictment of German barbarity.

DAVID B. TYLER

ANNUAL REPORT OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION, *showing the Operations, Expenditures, and Condition of the Institution for the Year ended June 30, 1954. Publ 4190.*

United States Government Printing Office, Washington. $3.00. x + 455 pp. + 76 pl.; text ill. 1955.

In addition to the usual reports from the branches of the Smithsonian Institution, the Yearbook contains in its General Appendix as excellent a selection of general scientific articles as in previous years, or as is to be found in any collection. Reprinted articles of biological interest are the following: On Comparing the Brain with Machines (D. M. MacKay); A Glimpse of Incomprehensibles (George W. Corner); The Electron Microscope in Biology (Ralph W. G. Wyckoff); The Migration of Mammals (L. Harrison Matthews); The Flight of Animals (James Gray); The Romance of Domesticated Plants (Glenn W. Blaydes); The Scientific Detection of Crime (Charles Sannie); The Great Piltdown Hoax (Wm. L. Straus, Jr.); and Medicine, Warfare, and History (John F. Fulton). In addition there are three original biological articles worthy of note: The Spread of the Cattle Egret (Alexander Sprunt, Jr.); Botanical Studies in Fiji (Albert C. Smith); and Shanidar Cave, a Paleolithic Site in Northern Iraq (Ralph S. Solecki).

CONCERNING THE NATURE OF THINGS. *Six Lectures Delivered at the Royal Institution.*

By William Bragg. Dover Publications, New York. $1.25 (paper); $2.75 (cloth). xii + 232 pp. + 32 pl.; text ill. 1954.

A photo-offset edition of the 6 Royal Institution lectures given by Sir William Bragg in 1923–24 on the nature of atoms, gases, liquids, and crystals.

CATALOGUE OF THE ROCK COLLECTIONS *In the Mineral Department of the British Museum (Nat. Hist.), Arranged Geographically. Part III. Antarctica and Australasia.*

By W. Campbell Smith and P. M. Game. British Museum of (Natural History), London. 12s. (paper). vi + pp. 165–255. 1954.

THE QUARTERLY REVIEW OF BIOLOGY publishes critical reviews of recent researches in all of the special fields of biological science. The contribution should present a synthesis or digest of the researches and a critical evaluation of them. A mere synopsis of the literature without evaluation or synthesis is not desirable.

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The article should be written in concise language, yet in sufficiently non-technical form as to be intelligible not only to specialists in other fields but to the general biologist as well. To this end the article should have a general introduction and a summary which enumerates one by one all of the principal facts and conclusions given in the paper. Interpretative diagrams and schemes are very desirable.

Material ordinarily taking the form of footnotes is set in small print and placed in the text and consequently should be written in a style so as to fit readily into the text. Acknowledgments are printed in the text in small type at the end of the article just preceding the List of Literature. Recent issues of the Quarterly should be examined for style as regards (1) section or subsection headings in the text, (2) literature citations in the text, and (3) List of Literature.

The subjects and authors of articles are selected by the Editors and members of the Advisory Board. Unsolicited articles which conform with the objectives of the Quarterly will be considered for publication.

A feature of the REVIEW is the section dealing with *New Biological Books*. In this department the book literature of different countries in the field of Biology is given prompt notice.

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